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THE PRESIDENTIAL ADDRESS

WORLD POPULATION AND WORLD FOOD SUPPLIES

BY

SIR JOHN RUSSELL, O.B.E., F.R.S.

THERE is an old Egyptian saying that whoever has once drunk of the waters of the Nile will want to go back and drink them again. I do not know if that has ever been said of the waters of the Tyne, but it is certainly true that the British Association always welcomes an invitation to Newcastle. This is our fifth visit; to no other city have we been more often.

We came first in 1838 when England was mainly an agricultural country. The 111 years that have elapsed have seen profound changes in our national life, particularly the prodigious advancement of science and of its daughter technology.

Among the many resulting achievements has been a great increase in the certainty and the length of life. The world population has risen more rapidly than ever before; it is now estimated by the Food and Agricultural Organisation at about 2,300 millions, and the increase at about 20 millions a year—an average addition of two every three seconds day and night, year after year, and the two may become three or more as science advances, social services improve, and bodies such as the World Health Organisation become fully operative. Can they all be fed? The problem is complicated by the inequalities in the rate of increase, the less advanced groups multiplying more rapidly than the more advanced, and also by the unequal distribution of population over the earth's surface. Over most of the world the population is so sparse that it can produce its own food; some regions can produce a surplus; but a few regions are so densely crowded that they can themselves produce only a restricted dietary—England, Belgium, Egypt, India and Eastern Asia. England is the most crowded but its highly developed export industries enable it to import food from the surplus countries, so long as they continue to have surpluses. India and Eastern Asia have fewer possibilities of imports, but their home production can be considerably intensified. Expansion of the world's food supplies depends therefore on extension of the food producing area, more intensive cultivation of the area already used, and the reduction of the present heavy wastes and losses.

EXTENSION OF THE FOOD PRODUCING AREA

The world's total land area is 35,700 million acres, but only about 11,000 million are estimated by C. B. Fawcett to be 'climatically suited' to crop growth. Much of this, however, is at present impracticable for cultivation, and the area actually used is some 3,000 to 4,000 million acres: 7 to 10 per cent. only of the world's land surface. Some 85 or 90 per cent. of this area is used for food production; the rest is for industrial crops. The food producing area represents a world average of about 1½ acres per head which, at British yields, would give the British pre-war dietary; much less would suffice for a vegetarian dietary. The area 'climatically suited,' however, is about 5 acres per head. Can any of those unused 3½ acres per head be brought into cultivation?

Agriculturists have long been engaged in widening the zone of cultivation by converting the marginal regions of their day into farm land and then pushing out into new marginal regions. The pioneers began in the coastal regions of North America, Australia and New Zealand, where conditions resembled our own, using methods that had been very successful here. Wheat was their main crop. But in pushing inland they were brought to a standstill by the diminishing rainfall, and in Canada the early onset of winter; everywhere their crops suffered from pests and diseases. In 1898 Sir William Crookes, in his Presidential Address to this Association, startled the world by declaring that the process of expansion was nearly over and by the 1930's the world would suffer hunger unless yields were raised. He showed how the necessary nitrogenous fertilisers could be prepared synthetically from the nitrogen of the air. Agriculturists did not accept his view. Lawes and Gilbert, old, but full of vigour and Victorian pugnacity, did what many indignant Englishmen have

done before and since, wrote to *The Times* about it. But, in fact, the address was one of the most momentous the Association has ever heard ; it founded the great nitrogen industry and everywhere stimulated agricultural investigators to fresh efforts.

The wheat problem was solved, but not in the way Crookes expected. Shortly after his address the new science of Plant Genetics was founded and for the first time it became possible systematically to transfer characters from one plant to another and so obtain new varieties possessing certain desirable qualities. Early ripening, shortness of straw and a high ratio of grain to leaf are all required in drought and frost resistant varieties, and all are heritable characters, as is resistance to rust and other diseases. Worldwide search was made for varieties possessing the desired genes, pure line and other techniques were evolved, and new varieties were in time produced suitable for the marginal regions so that these became productive farm lands. By the 1930's Canada's 1898 wheat acreage of 4 million had risen to 27 million ; Australia's 5 million to 16 million, and Argentine's 7 million to 20 million ; the world acreage and output rose far beyond the limits set by Crookes, and there was such a glut that wheat growers everywhere suffered greatly and relief measures became necessary ; in the Argentine wheat was burnt in the locomotives instead of coal.

The dreaded catastrophe had not eventuated but it had been averted by means wholly unexpected ; one of agriculture's many illustrations of the ancient truth—

> 'And the things that we looked for, the gods deign not to fulfil
> And the paths undiscerned of our eye, the gods unseal.'

Since the 1930's, however, the wheat area of these new countries has not expanded, which has been taken to show that no more new wheat lands are available. The deduction is unjustified. Agricultural development of new lands is very costly, requiring development of transport, storage and marketing facilities, and it is not undertaken unless a profitable market for the produce is assured. The glut of the 1930's had a restraining effect on development. Even the war has not stimulated much expansion. In 1940 Canada raised her acreage to 28·7 million in response to our urgent appeal, but the difficulties of disposal caused such discouragement that in 1941 the area fell to 21·9 million acres, and even now is only about 24 million acres, this being all that present conditions justify. All the same, Canada was able last year to supply three-quarters of our imports of wheat and flour. We owe a great debt to Canadian cerealists who made this possible—L. H. Newman, C. H. Goulden and their associates —and to the agricultural engineers who excelled themselves in devising implements for cultivating the soil, sowing and harvesting the crop with the minimum of manpower.

A new and very promising attack on the marginal lands is now being made. It is the soil-climate complex, not the climate alone, that determines agricultural use. The climate factor may be beyond out control but the soils are being improved. Marginal lands are generally used for ranching—often many acres to one animal. Their present use is for low productive ranching only. The more tractable are being converted into properly managed grazing land. The approach is ecological ; the vegetation is surveyed, the types classified, methods of management and conservation are worked out for each type. In South Africa over 100 different types of Veld are recognised, each requiring its own system of management. Seeding with better grasses is being widely practised, particularly in the United States ; the world is ransacked for suitable types. Canada and the United States took crested wheat grass from Russia ; Australia has taken grasses from the Mediterranean countries, Africa, Turkestan and India ; they are grown at the experimental stations, suitable strains selected and multiplied. Cross-breeding has begun. Good work is in progress at Saskatoon, Swift Current and Kamloops in Western Canada, and by Australia's vigorous Division of Plant Industry and, of course, in the United States. Unfortunately some of the good drought-resisting grasses do not easily form seed under cultivation, though they do so readily in their wild habitat. The cross-breeding of the semi-arid types has hardly started, though collections such as that of I. B. Pole Evans in South Africa, D. C. Edwards in Kenya and others afford good material. In view of the very great importance of grass for soil conservation in all parts of the Commonwealth, it is highly desirable that a well-equipped Commonwealth station should be established to study existing semi-arid types and produce new ones. One of the great needs now is for leguminous plants that will do for the semi-arid and tropical regions what subterranean clover and lucerne have done for the moister regions.

Large tracts of land hitherto unfertile are gradually yielding up the secret of their barrenness. Australia has made great progress. In large parts in the South the cause was the lack of trace elements : zinc, copper, molybdenum and others ; these being supplied, the soil under proper management became productive. In the attractive Albany region are 300,000 acres to be made into

400-acre farms. For the large parts in the North, however, the secret is not yet discovered, nor has it been for the 300 million acres of peat and muck soils in Canada. But in both countries competent scientific workers are studying these problems under the organisation in Australia of Sir David Rivett, and in Canada of Dr. E. S. Archibald, E. S. Hopkins and others. Soil barrenness in New Zealand is ably investigated by Sir Theodore Rigg and his colleagues at the Cawthron Institute.

The wheat belt which once stopped at about the line of 18 inch annual rainfall, now stretches to the 12 or 15 inch line (dependent on the evaporation), and the utilisable grass belt down to the 8 or 10 inch line. There are, of course, difficulties in developing regions of erratic rainfall, particularly the old Joseph problem of storing the surpluses of good years for use in bad years, but these should not be insuperable.

The hot regions of the world present special problems because they can never be closely settled by white people. Australia's empty North has vast areas of natural grass land with adequate rainfall for beef production, and bull-dozers have revolutionised the making of dams for water storage. The development problems are discussed in an excellent report by W. L. Payne and J. W. Fletcher (1937). They include land and tenure reform, pasture improvement, better living conditions for the resident workers, better abattoir facilities and transport. A chilling process based on the low-temperature research at Cambridge can now replace the old unsatisfactory freezing technique whenever the quality of the beef justifies the cost. The possibilities of arable cultivation have been greatly increased by the development of the disk plough and other implements; the Overseas Food Corporation is proposing to develop 315,000 acres of Northern Queensland, mostly for arable crops, including Sorghum varieties suited for mechanised culture.

Africa's problems are fundamentally different. The small native peasant is already in occupation with his own traditional ways of life. Yet he alone can supply some of the products vitally important to us—oil seeds, fibres, tea, coffee and cocoa. The problem is to replace his primitive methods by modern efficient methods without too great disturbance of the best elements in the native life. We may be justly proud of the splendid pioneering work of the British agricultural officers; this country has done more than any other for the opening up of Africa. The Sudan Plantation Syndicate has for many years been highly successful in combining modern scientific cultivation methods with a way of life acceptable to the native and justifiable before the bar of civilisation. In West Africa progress has gone so far that mechanisation can now be introduced into the native community farming: this has been discussed in the excellent Clay-Monson Report. As in Australia the bull-dozer's remarkable power of scooping out 'water holes' promises greatly to facilitate development. In Central and East Africa scientific workers at Amani and in the Departments have paved the way for the huge scheme of developing 3¼ million acres of savannah land, hitherto empty because of the tse-tse fly and lack of surface water—troubles now capable of reduction. The unfortunate decision to rush this scheme through has been responsible for much costly disappointment for which, however, the scientific and technical staffs are in no way to blame. Progress would become still more rapid if a good livestock industry could be developed; this would necessitate not only better animal health but more and better food supplies for the animals. The native problems have been lucidly discussed by Sir Alan Pym in his book *Colonial Agricultural Production* (1946). The fundamental scientific problems are investigated here under Sir John Simonsen and the Colonial Office Research Council, and in East Africa by a research team under B. A. Keen. Development organised by the Colonial Office is proceeding more rapidly and systematically than ever before.

Difficult human problems are bound to arise, however. The high costs necessitate high output, and the conservation of the soil requires strict control of land use over wide areas. Close supervision by advanced white people will be essential until Africa has its own highly efficient Universities, research workers and chemical and engineering industries. African students have been brought over here for higher training, but for some their stay has been made unprofitable by inadequate social arrangements, or even by hostile political propaganda.

The wet tropics (50 inches or more rainfall per annum) contain more unused land than any other vegetation region in the world. Sir Harold Tempany has discussed the possibilities of the British portion; the Belgians have done much useful work in the Congo; and Unesco proposes a survey of the vast forest basin of the Amazon. Dr. Kellog and other American experts are hopeful about the utilisation of part of these regions.

At the other end of the temperature scale lie the cold leached soils of the northern latitudes, the podsols, naturally under coniferous forest but brought into cultivation as need arises. The Finns—the most northerly farmers of the world—the Canadians and the

Russians are all studying the problems involved, while the Swedes, at their incomparable station at Svalof, have produced varieties of arable crops well suited to the conditions.

But we need not go abroad for marginal and waste land ; we have plenty at home ; this also can be reclaimed by conversion first into grass land, then if need be into arable. Sir George Stapledon and his colleagues at Aberystwyth have produced suitable strains of grass and clover ; their use is investigated by William Davies at Drayton. R. Ellison has reclaimed much waste hill land in Montgomeryshire and estimates that 1,000 acres so reclaimed could provide for an additional 250 store cattle. Some half million new acres were brought into cultivation in the nine years ending June 1947.

Even light sands, long abandoned as waste or left to the foresters, are now being put down to suitable grasses or lucerne and used for dairying. Norfolk and Suffolk have some excellent examples of reclamations brought about by enterprising farmers and landowners. *Spartina Townsendi* shown in 1923 by F. W. Oliver to Lotsy and Verhoeven as a reclaimer of mud flats has proved very successful in Holland and may yet provide much new land there and here also.

In short, wherever cultivation is now practised, there are possibilities of extension and no one can say how great they may be.

Soil Erosion

Against these potential gains, however, must be set the very serious losses due to soil erosion. This was really an old trouble to the Mediterranean and Eastern peoples, but it was new to the agricultural pioneers who went out from this country and Western Europe, and they did not realise how greatly the regions to which they were going were liable to it. Unwittingly their practices favoured it : uncontrolled felling of trees in the moister regions led to water erosion, and in the drier regions exclusive grain cropping with finely cultivated fallows to destroy weeds and conserve soil moisture—a false expectation based on the faulty Soil Physics of the time—broke down the soil crumbs into fine dust which was blown away by the scorching winds of the hot dry summer.

In Africa great losses have long resulted from uncontrolled burning and over-grazing the grass lands, exposing the bare soil to the beating of the torrential rains characteristic of semi-arid regions, washing it away and often leaving great gullies ; and matters were made worse by the success of the Health and other Departments in increasing the human and animal populations. It was the dust storms of the dry 1930's in the United States, and in particular a terrifying one on May 16, 1934, that made all America aware of the ' Dust Bowl ' and the need for drastic action. In the United States some 330 million acres out of 460 million acres of good arable land were estimated to have suffered, as had something like half of the wheat land of Australia, great areas in Africa, Asia and elsewhere. A useful account is given in G. V. Jacks' and R. V. Whyte's *Rape of the Earth*, but there has been much sensational writing which may, however, have stimulated action.

The United States was the first to act and set up very successful Soil Conservation services and a superb model : the Tennessee Valley Authority.

It is now recognised that erosion comes about through wrong use of the land, and the remedy is correct utilisation, both of land and of water, applied over the whole catchment area. The problems are therefore not exclusively technical but administrative also. A vegetation plan must be drawn up to ensure continuous cover of critical areas—hilltops, slopes, etc.—with trees or grass, and the protection of arable land against soil movements and run-off of water by mechanical devices such as contouring, alternate strip cultivation, etc. For full effectiveness the plan requires the collaboration of the ecologist, the agriculturist, the forester and the plant breeder ; the ecologist to ascertain what plant combinations are most appropriate, the agriculturist and forester to select those of most economic value, and the plant breeder to narrow the gap between the ideal plants and those actually available. Grass plays a vitally important part ; it not only covers and protects the soil but builds up the fine particles into crumbs by processes not yet understood ; successful treatment largely depends on the selection of the right grasses, particularly rhizomatous or stoloniferous types. A large amount of successful conservation has been done and much eroded land has been rehabilitated. The United States Conservation service under H. H. Bennett now operates over some 1,100 million acres ; three-quarters of all the farm and ranch lands. The ' Dust Bowl ' States—Oklahoma, Kansas and Texas—are now producing much more wheat than they did before the ' Bowl ' was heard of. The very sound Canadian Prairie Rehabilitation programme is in vigorous operation ; in all the Australian States affected—especially New South Wales and South Australia—there are active Soil Conservation Departments, and W. Hartley of the Division of Plant Industry is studying suitable grasses. Much good work has been done by I. B. Pole Evans in South Africa, W. S. Martin in East Africa, R. R.

Staples in Southern Rhodesia, and by others in the Belgian Congo and elsewhere.

Soil erosion could be dealt with much more easily if the problems were solely technical ; unfortunately, the need for recasting the vegetation of a whole catchment area often cuts across all sorts of social or tribal customs, personal or sectional interests, political boundaries, etc., and raises many complex administrative problems. A few recalcitant objectors could spoil the whole plan ; it must be enforcable by law, but voluntary co-operation of all concerned should be the aim. The United States Soil Conservation laws achieve this admirably and have furnished models to other countries. The farmers and landowners of the region are invited to meet the Government and State experts and a plan for proper land utilisation is drawn up ; any necessary material help is given. The farmers, feeling that the plan is at least in part their own, support it ; there is not the automatic opposition given to a plan imposed from above. South Africa also has a comprehensive Conservation law. The over-populated and over-grazed areas of Africa present special difficulties where the remedial measures conflict with treaty rights and tribal customs ; agitators have not scrupled to stir up trouble, and much interference with useful work has been reported in the native areas of Kenya. Fortunately, the white Governments concerned—our own, the South African, Rhodesian, French, Belgian and Portuguese—recognising the truth of General Smuts' dictum that soil erosion in Africa transcends all political problems, have agreed to co-operate and to set up an information office in Paris and a soil research organisation in Africa. The many problems met with in the British Colonial Empire have recently been ably discussed by Sir Harold Tempany.

An elaborate plan has been drawn up for dealing with erosion in the U.S.S.R. In most other affected countries, except perhaps China and parts of South America, the need for control is recognised and the lines of action laid down.

Rehabilitation is necessarily slow, and there are no good figures to show how far it has gone. The estimates currently quoted are those of the early 1930's when the need was just being recognised and the bias was towards all-inclusion. It is therefore impossible to say whether rehabilitation has yet caught up with erosion. But those engaged in the work are hopeful about the outcome ; the control measures are known to be effective. Moreover, erosion is associated much more with extensive and mono-culture systems of agriculture, than with the intensive mixed farming systems that will necessarily be more widely adopted as the demand for food increases.

Intensification of Cropping

The most hopeful way of increasing world food supplies is by more intensive cultivation of the land already in use. It is especially important for Asiatic and African peoples—60 per cent. of the world's population—for they have no prospect of emigration and very little of food imports from other continents. Their present product is chiefly grain, the yields low and uncertain, the methods inefficient. Experimental farms in India commonly obtain twice the yield of the neighbouring peasants, and there is often no physical reason for the disparity. But the difficulties of levelling-up are enormous ; they spring from the peasants' poverty, their overflowing fecundity and the consequent minute sub-division of the land ; these can only slowly be remedied.

Meanwhile, some improvement is possible. Irrigation can still be considerably extended. The projected Nile schemes, if realised, will solve Egypt's problems for a generation, while Asia, which already has some 70 per cent. of the world's irrigated area, mostly in the Himalayan region, can greatly increase the area served by tube wells as developed by Sir William Stampe.

Asia also offers vast scope for fertiliser use. Rice is the crop most affected ; it is perhaps the most crucial crop in the world, for it is the main food of Asia and its shortage is one cause of unrest there. It has been much studied in India and improved varieties have been raised by pure line selection ; hybridisation is hardly begun. Nothing would more greatly increase its output than wider application of nitrogenous fertilisers. But rice is mainly a swamp crop and ordinary methods of fertiliser use break down. Nitrate of soda is relatively ineffective : sulphate of ammonia often acts well but sometimes not. These puzzling complexities were resolved from a quite unexpected quarter. Professor Pearsall in studying marsh conditions in Cumberland found measurable differences in oxidation—reduction potential in the water-logged soils ; oxidation took place at the soil-water surface but reduction lower down. Directly they heard of this, the Japanese experts saw its bearing on the rice problem. Sulphate of ammonia put on the surface of the mud in the usual fashion, at or before transplanting, was quickly oxidised to nitrate which washed down below and was reduced to gaseous nitrogen. But if the sulphate of ammonia was pushed down through the oxidation zone into the region of reduction it lay safely till the plant roots took it up. If nitrates, e.g. ammonium or sodium nitrate, are added

during the period of rapid growth they can be absorbed quickly before they reach the reducing layer ; therefore they may be used as later top dressings. The study of these oxidation and reduction zones showed also how cyanamide and other fertilisers should be applied. It gives for the first time a scientific basis to the manuring of the rice crop ; another example of the far-reaching results that may come from a purely academic enquiry well carried out.

The human problems are more difficult. Will Burma under her new management retain her leading position[1] among rice exporters ? And can Siam and Indo-China produce as efficiently as before ? How will India fare if they do not ?

The millets are specially important as native food in the semi-arid regions of India and Africa. We have done less work on these than we should, but this is to be rectified in the East African research scheme.

The general methods of intensification of food production are the same everywhere. Land tenure and size of holding usually need modifying to encourage higher efficiency : the holding should be of such size that one man can adequately supervise it. More productive systems must be evolved, and bare fallows replaced by tilled crops and by grasses and leguminous plants. Inevitably there comes a development of live stock. In the highest and most productive systems live-stock products yield far more income than cash crops ; before the war our live stock accounted for 70 per cent. of the value of our farm produce, and according to Leitch and Godden's estimate, consumed on the average about 100 million million calories per annum; just about double the consumption of all our human population ; and giving us in return less than ten million million calories. But it is only fair to say that they supply other essential needs, that most of the food stuffs they consume are useless to us, coming from grass, straw and various industrial and other residues ; and that they greatly enhance the productiveness of our soils.

Intensification is greatly facilitated by the adoption of some form of farmers' association allowing joint and economical use of complex appliances, buying and selling operations, etc. In Denmark co-operation, and in this country a variety of contract and co-operative methods, have been used with great success ; in the U.S.S.R. collectivisation is adopted. Whatever the method, it must suit the psychology of the farmers and be freely adopted by them if it is to have much chance of success.

[1] Before the war Burma exported almost 3 million tons of rice annually out of a total export of more than 7 million tons usually from South and East Asia.

Chief among the intensification methods is the improvement of the soil by fertilisers, lime, drainage, etc. Fertiliser requirements are ascertained by chemical analysis and by statistically controlled field trials as used by E. M. Crowther and F. Yates at Rothamsted or by foliar diagnosis and plant analysis, as used by T. Wallace and his colleagues at Long Ashton, and for sugar cane by Pierre Halais in Mauritius. Trace elements are studied by spectroscopic methods developed at the Macaulay Institute, Aberdeen, the Waite Institute and the Division of Soils, Adelaide, and elsewhere, while W. A. Roach at East Malling has ingeniously tackled the problem of supplying them.

Happily the necessary fertilisers are producible in almost unlimited amounts ; the nitrogen comes from the air to which in due course it returns ; the known deposits of phosphate are estimated to suffice for some thousands of years at present rates of consumption ; those of potash are also abundant. So far only Western Europe and ourselves use much fertiliser, between us we took nearly half the world's pre-war production.[2] The war has brought about a great increase in consumption ; both in this country and in the United States this has doubled since 1938. World consumption of nitrogen before the war was 2 million tons annually ; the present declared need is for about 4 million tons and the factory capacity is of the same order.[3] In moist regions nitrogenous fertilisers have more than fulfilled Crookes' expectations ; they saved us from hunger in the two wars, and with clover and lucerne are our best source of the protein equivalent necessary to produce the milk and meat we so greatly need. In drier conditions phosphates are more effective ; they have greatly improved the Australian and New Zealand pastures and furnished us with large quantities of much needed lamb and butter. Much more fertiliser could advantageously be used if farmers' means permitted. A better fertiliser supply and more leguminous cropping would quickly raise output over large parts of the world.

Along with more fertiliser goes the need for better seeds and better varieties ; here the British farmer is particularly fortunate.

But all improvements fail unless weeds—

[2] The consumption per acre was in the following order : Holland (highest), Belgium, Germany, Denmark and Great Britain, France, and the aggregate was 846,000 tons of nitrogen and nearly 1½ million tons of P_2O_5 out of a world consumption of just over 2 million tons of nitrogen and 4 million tons of P_2O_5.

[3] Excluding U.S.S.R., Northern Korea and the Russian zone of Germany for which no figures can be obtained. The consumption figures (F.A.O.) include technical requirements which, before the war, amounted to about 10 per cent. of the total.

arch enemies of mankind—are adequately controlled. Their destruction is one of the chief tasks of arable farming; chemical methods are coming in but cultivation is the standard method; this and the preparation of a seed bed are usually its main purposes. Research on implements is done at the Farm Machinery Institute at Silsoe under W. H. Cashmore, and on field effects by E. W. Russell at Oxford.

Perhaps the most significant changes in modern farming have been the increasing mechanisation and the alacrity with which the younger farmers have accepted it. Tractors are displacing working horses—perhaps too rapidly.

Before the war about 60,000 tractors and 650,000 horses were used in Great Britain's agriculture. In 1948 there were over 260,000 tractors, mostly ranging from 12 to 40 h.p.,[1] a gain of more than 200,000; in the same period horses were reduced by 192,000. Assuming with the Oxford Agricultural Economics Research Institute that the effective working power of one tractor is, on the average, the equivalent of four horses, our farms had in 1948 1½ million 'horse equivalents' compared with the pre-war 889,000, a gain of 68 per cent. During the war the quantum of output per man-year rose about 10 to 15 per cent., but the evidence available for the past five years suggests that, despite additional power and machinery, annual output has been below the war-time peak while the total labour force on farms, allowing for changes in composition, has remained well up to war-time strength. Clearly there is room for more economical use of farm power.

It is impossible to forecast what the tractors of the future will be like; the United States already has some of 140 h.p. The limit is likely to be set by the amount of oil fuel available; the French foresee a shortage and are starting experiments on the production of methane by fermentation of straw. Tractors require their own special implements, which must be generalised and mass produced to keep down their cost; they can no longer be made by local craftsmen to suit local requirements like the old types; their manufacture is shifting to the industrial regions of the Midlands and Yorkshire—Canadian and American firms are recognising the possibilities.

These engineering developments are enabling farmers to cope with the ancient difficulty that Nature's production of food is by jerks, while consumption is fairly steady. Storage is therefore necessary; usually it has to be done on the farm and it is often attended by considerable loss. Grain crops may suffer from rats and mice; these are estimated to destroy 2 million tons of food annually in England and Wales alone; a single rat is said to consume 50 lb. of grain a year and to spoil an additional 1½ cwt. Grass, our most important and for long most neglected crop, when made into hay commonly loses 30 per cent. or more of its valuable protein equivalent, according to S. J. Watson's estimate; but in any case haying is inapplicable for modern intensively managed leys or lucerne that give three heavy cuts of highly nutritious material a year. Artificial drying preserves the nutrients intact; it requires 10 to 15 cwt. of coke per ton of product; 3 tons of dried grass per acre can be obtained—three times the old average yield of hay. Ensilage is cheaper, but involves loss of 20 to 30 per cent. of the nutrients; lack of driers makes it at present the more practicable alternative.

Increased intensification necessitates strict measures against pests and diseases which now travel widely and are favoured by the greater crowding of crops. Resistance is fairly frequently a heritable character, and once the genes are found, resistant varieties can be produced, such as the rust-resistant wheats of North America and Australia, and the potatoes bred at Cambridge and at Corstophine. The pathogen may exist in several strains, necessitating the introduction of several genes. The work is never ending, for a relatively harmless organism may suddenly change, like the Verticillium which, having long lived an almost blameless life in the Kentish hop gardens, about twenty years ago suddenly produced a virulent mutation which has since done much damage there.

Measures for protecting plant and animal health are being steadily improved; they are the subject of important international Conferences; if in spite of all precautions the attack still comes, counter-measures can often be taken. Plant virus diseases still defeat us, but the able and persistent efforts of F. C. Bawden, N. W. Pirie, Kenneth Smith and others cannot fail to bear fruit. Against insects the new chlor- and phosphorus-insecticides are very potent and methods of spraying have greatly improved; even wireworms, long invincible, have succumbed to hexachlorbenzene. But there are always risks in interfering with Nature; the D.D.T. which was so effective against codling moth in orchards, also killed the insect that had kept in check the red spider, and this not being affected by the D.D.T. became in its turn a pest.

Animal diseases—to which we are very liable because of our large imports of animal carcasses—are steadily being brought under control. The old method was instant

[1] The tractor statistics include market garden types of lower h.p.

slaughter to avoid spread ; it was effective but costly. The modern method is to apply diagnostic tests so as to catch the disease early, then to segregate and give the appropriate treatment. Vaccines are used for a number of diseases and immunisation by the appropriate serum is growingly practised. Estimates of loss are always rather speculative, but before the war the loss of milk due to disease in England and Wales was estimated at 200 million gallons per annum. More definite is the fact that the present life of a cow in the milking herd averages about 3½ lactations only ; it should be 6 or 7.

Most countries could increase their food production considerably by applying known methods of improvement and raising the average performance to the level of the best. The yield of wheat in Great Britain used to be about 8 or 10 cwt. per acre ; it now averages 20, and the best farmers get 30 or 40 ; but the world average is only about 7. A pedigree breeder may export a bull for £3,000 ; a scrub bull may be dear at £30. The average milk yield of England and Wales is about 550 gallons ; recorded herds average about 700, and yields of 1,000–1,200 gallons per cow for a whole herd are not uncommon. Trained advisers, recently considerably augmented in number, have long been trying to reduce these gaps.

The old agriculture could be carried on by appliances made by local craftsmen. Modern intensified agriculture cannot ; it demands high-grade machines, chemicals, sources of energy that only a highly developed industry can supply and, of course, corresponding standards of education among the farmers. A food producer on the old system can feed about 4 to 5 persons—this is still a very common number ; on modern systems he can feed about 15 or 20, provided some of these supply him with the necessary appliances. In the self-sufficient countries of Western and Central Europe about 20 to 30 per cent. of the population is agricultural ; in Eastern Europe and in Asia the proportion is 60 to 70 per cent. or more, and the standards of living are much lower. They can still be raised, but marked improvement can be obtained only by taking some of these redundant people off the land and setting them to make the appliances for a better agriculture, and providing the necessary transport and buildings.

But this raises new problems ; it involves segregation, as seen very clearly in England where the difference in outlook between town and country is sharply marked, and knowledge of the countryside is often curiously lacking. So we hear exhortations to more food production, but at the same time the land on which alone this can be done and the money necessary to do it are steadily taken away for other purposes. Some land reclamation, it is true, has been done ; in the nine years 1939–1947 the area of rough grazings in England and Wales fell by 1·4 million acres, much being brought into cultivation. But a larger area, often of better land, went out of cultivation and the total land under crops and grass shrank by 316,000 acres according to the official statistics.[1] It is hard to find a countryman with a good word to say for planning. In the end common sense has a way of prevailing and we may still look for further intensification of production even in our highly cultivated land, still more in the less developed regions elsewhere. From our present small and shrinking allocation of land we may yet raise half our food, besides improving the quality of both meat and milk. And the less developed countries can feed their people better than they now do.

New Uses of Science in Food Production

Pure science is continuously opening up new possibilities for use in agriculture. The starting point is commonly some purely academic problem of no apparent practical interest whatsoever. R. A. Fisher's investigations on the theoretical foundations of Statistics, ably developed by F. Yates, have revolutionised agricultural experimentation by enabling investigators to make valid estimates of the significance of their results. Frequently the physiologist has started the chain of enquiries, followed by the biochemist and the organic chemist, and finally the technological chemist.

One of the most fruitful series of investigations began with the old enquiry : Why do plants turn their leaves towards the light? Went and Kögel at Utrecht showed that the response was due to minute amounts of an organic compound which operated by causing growth of the tissue where it happened to be. Several of these growth promoters were found ; they are called auxins ; they have a fairly simple chemical pattern ; a straight-chain acid group combined with a benzene, naphthalene or indolyl group : indole-3-acetic acid is one of the commonest.

These and similar substances were soon prepared on the large scale and were shown to promote root formation in cuttings: Zimmerman and Hitchcock at the Boyce Thompson Institute investigated the action in detail and horticulturists used it in

[1] Professor Dudley Stamp puts the net loss at 438,954 acres, after allowing for some 500,000 acres of reclamation. The difference largely turns on the definition of 'cultivation.' In the official figures for 1948 the ten year fall is given as 270,000 acres.

practice ; these and other effects are being followed up at East Malling under F. G. Gregory. Later it was found that these auxins induced the setting of fruits of the tomato and cucumber family without the intervention of pollen from the male organ ; Wisley, Long Ashton and Wye have all been active here. A spray of β-naphthoxyacetic acid, 40 parts per million of water, has been used considerably by commercial growers ; the fruits are normal in appearance but not yet in quality, and they are seedless. Some 50 substances of this type have been tried by R. L. Wain and Daphne Osborne at Wye, and L. C. Luckwill at Long Ashton ; each has some effect on the habit of growth of the plant or the character of the fruit—not always desirable—but a considerable range of control is possible.

So far no substances have been found effective for large fruit trees except some varieties of pear.

Another important action of the auxins was discovered by W. G. Templeman, E. H. Slade and W. A. Sexton at Jealotts Hill, and somewhat later, but quite independently, by P. S. Nutman and J. H. Quastel in following up some of H. G. Thornton's work at Rothamsted. It was naturally expected that auxins might increase seedling growth ; but they did not, they even killed some but not others. Cereals were much less injured than some of their common weeds, and selective weed killing became a possibility. Chlorine atoms were introduced to give sufficient stability and greater toxicity : 2 : 4 dichlorophenoxy-acetic acid (called for short 2 : 4 D) is very effective and now widely used, especially in North America : some 5 to 10 million acres of grain were expected to be treated in Canada last year. G. E. Blackman at Oxford with a team of workers is developing this chemical control and important practical results may be confidently expected.

Others of these compounds are used for checking growth ; preventing potatoes from sprouting in storage ; staggering pineapple harvests in Hawaii to fit the canning facilities; preventing the pre-harvest fall of apples, etc. Other substances change vegetative shoots into flowering shoots. No one can see the end ; and, I repeat—it all started with the purely academic enquiry : Why do plants turn their leaves to the light ?

Another investigation that may have far-reaching effects is being done jointly by a team of workers under R. Brown of Leeds and A. R. Todd at Cambridge. Brown and M. Edwards had shown that the seeds of *Striga*, a plant parasitic on Sorghums and millets and the cause of much loss in Africa and India, germinate only when stimulated by a substance excreted from their roots. Todd found it to be apparently a sugar : D-xyloketose acted well. If the different varieties of Sorghum should differ in their ability to prepare and excrete this stimulant, a chemical test might be devised that would greatly assist plant breeders searching for resistant strains.

It is possible that other parasitic fungi can live only on varieties of plants capable of producing the necessary stimulant.

The cysts of the destructive potato eelworm hatch out only when stimulated by a substance excreted by the roots of potatoes, tomatoes and other Solanaceae ; this also is being studied by A. R. Todd. When this is identified and prepared on the large scale, it might be applied to the land while fallow, so that the emerging larvæ could perish of inanition before the potatoes were planted.

Various soil organisms are known to produce antibiotic substances, analogous to penicillin, highly destructive to certain other organisms. Thaysen and others have found in tropical soils bacteria with high powers of inhibiting the growth of fungi. In the absence of a good soil fungicide, it is possible that soil-borne fungal diseases such as ' Take-all ' disease of wheat and Panama disease of bananas might be successfully controlled by introducing or encouraging organisms producing the appropriate antibiotic. Bacteriophages, destructive to various bacteria, are also known to exist in soils though they have as yet found no application in agriculture. Add to this list the large number of protozoa, acrasieae and giant rhizopods found in soil by B. N. Singh and other Rothamsted workers, and shown to feed on bacteria and other soil organisms, and it is clear that life within the soil is as full of risks and of possibilities as life on the surface. Some degree of control of this vast soil population so vitally important to soil fertility has long been sought, and the older method of partial sterilisation may in due course be supplemented by newer agencies.

Some of the algae and fungi, especially the yeasts, have remarkable power of synthesising fats and proteins. Only limited use has so far been made of this property but the possibilities are considerable. Dr. H. A. Spoehr and his colleagues at the Plant Biological Institute at Stanford, California, showed that the composition of the dry matter of *Chlorella* could be varied widely by suitably modifying the environment ; the fat could be raised to 50 per cent. or the protein to 75 per cent.

Moreover, there still remain unknown possibilities in the oceans, for they carry much more vegetation than the land. My colleague Prof. Newton, is dealing with this

matter in her Presidential Address to the Botany Section.

Genetical science can still do much more to help crop production. To give only one instance : wide crosses often produce sterile off-spring. But treatment with colchicine in some cases doubles the chromosome number and makes the off-spring fertile. New types can thus be synthesised which can be used for producing new economic forms.

Ecology is also coming in to help. The old idea that science would enable the farmer to go anywhere and produce anything has given place to the more realistic principle that every acre of land should, as far as possible, be used in the way to which it is best adapted. Extensions of the cultivated area, treatment of eroded land and improvement of wild pasture are already seen as ecological problems but the range is much wider and the possibilities great.

Many of the vital processes in animals are, like those of plants, determined by chemical substances, and some degree of control has already been attained. H. D. Kay at the Dairy Research Institute, Shinfield, has shown that thyroxin injected subcutaneously, or iodinated protein (casein) fed by mouth, increased the fat content of the milk and also raised the yield up to 20 per cent. after the peak of lactation was passed. Even more dramatic was the use of a synthetic oestrogen, diethylstilboestrol, introduced in tablets under the skin, for inducing lactation in virgin heifers or barren cows—the first stage in making the male redundant.

Artificial insemination is already widely used and is rapidly spreading, especially for dairy cattle, as milking capacity is largely inherited from the sire. Much successful upgrading has been done in Denmark ; some half million cows were inseminated in 1946, but this is only a beginning. J. Edwards estimates that by suitable dilutions the number of cows inseminated per bull could rise to 10,000 or even 15,000 per annum. We may yet live to see bulls, other than a small select aristocracy, become unwanted anachronisms. But that is not all. A cow normally produces only one ovum at each oestrum. When pregnant mare serum hormone is injected, however, she produces a number, as shown by J. Hammond, jun., and Bhattacharya at Cambridge. Attempts are now being made to transplant these into other cows—one into each—fertilise them by artificial insemination, and so get a dozen or so off-spring from the chosen mother in place of a solitary calf. Scrub cows, otherwise of little use, could thus become incubators of the eggs of high-grade cows : the method would be particularly valuable for beef cattle. Already the transplanting has been successfully done with rabbits. No end is in sight : one cow could potentially produce some 75,000 ova during her lifetime. Nevertheless, there is always the lurking suspicion that Nature may resent being unceremoniously bundled out in this way ; we can only wait and see.

For it is a fact that attempts to control natural processes without adequate knowledge are always risky and may be dangerous. And so agricultural scientists have long aimed at knowledge rather than control, certain that if their knowledge is sound, their control will be secure. In the great agricultural research institutions to-day ideas and equipment are freely borrowed from the pure sciences ; in studying soils, plants, animals and their relations one to the other, use is made of modern biochemical methods, of physical methods, of X-ray photography, of the electron microscope, of tracer elements to follow the processes of plant nutrition and so on. There is inevitably a time-lag between the gaining of new knowledge and its possible use ; for engineering Sir Richard Southwell puts it at 50 years. For agriculture it is less, and the Agricultural Improvement Council has been recently set up to keep it to a minimum. The Royal Agricultural Society has long been working to the same end.

I shall attempt no forecast as to how much food the earth could be made to yield. The limit at any time is set by the efficiency of the plant as a transformer of radiant energy ; at present this does not exceed 5 per cent. and, reckoned on the basis of the amount of food produced, it is much less. Whether this can ever be raised, whether we can ever do more than increase the proportion of assimilation products useful as food, cannot be said. But the present limitations to food production : utilisation of 7 to 10 per cent. only of the earth's surface ; conversion by the animal of 10 to 25 per cent. only of its food into human food ; and fixation by the plant of no more than 5 per cent. of the radiant energy it receives : these are all challenges to agricultural science—which its workers are vigorously taking up.

No limits can be set to the possibilities of science, but it does not follow that the present rate of advance will continue. Great scientific discovery is possibly only for the gifted few, and only in an atmosphere of complete intellectual freedom : subordination to political or any other dogma is utterly destructive. Nor can we praise the growing tendency to take good scientists out of the laboratory and put them into Committee rooms, or to load them up with so much administrative and other detail—nowadays housework as well—that they lack the leisure for ' reaping rich harvest from the mellow

soil of quiet thought, mother of great deeds,' to quote the noble words of Aeschylus. Then there is the ever growing specialisation against which Sir Richard Livingstone and others are tirelessly warning us. The individual suffers : Whitehead reminds us that a man may know all about the laws of light and yet miss the radiance of the sunset and the glory of the morning sky. And the loss to science may be greater : the breadth of culture of some of our pioneers—D'Arcy Thompson, Percy Frankland, J. B. Farmer and others—had much to do with their breadth of scientific vision.

Nevertheless remarkable success is attained by teams of highly trained specialists working under an inspiring leader—even when the team includes men who, by themselves, would easily become—

' lost in a gloom of uninspired research '

as Wordsworth put it.

The possibilities of food production are, however, only part of the story. Food producers will labour to obtain a surplus for the outside world only on condition that they are provided with adequate appliances and incentives. And the replacement of craftsmanship by mechanisation is inducing in agriculture, as in industry, a flight from labour. In many cases now the problem is not so much to increase output as to maintain markets and reduce hours of work. Happily, in spite of modern tendencies, a strong sense of individual responsibility in regard to hungry peoples still survives among food producers. This is, indeed, the basis on which Boyd Orr built up the Food and Agriculture Organisation, now doing such useful work. But it would be entirely wrong to think of the world as a Welfare State where food will be produced simply out of a sense of duty. If more food is needed, more work must be done to earn it.

The increased food production that may confidently be expected is more than ample for population increases at Western rates. But it could easily be outstripped locally where Eastern rates of increase prevail. A physiologically adequate dietary might still be attainable, but if Western standards of living are desired, Western restraints must be adopted.

These, however, are human problems, and, great as are the powers of science, it is of only limited help here. It can do much to overcome material difficulties and, better still, to satisfy man's thirst for knowledge of the Universe in which he lives, and it can insist continuously on our high duty to seek out the Truth fearlessly and honestly, and having found what we believe to be the Truth, to proclaim it—but in all humility and recognising that we may be wrong. Apart from that, science can give little guidance in those great moral and spiritual problems which lie at the root of our most serious troubles to-day. It opens up many possible ways of life but gives no help in choosing which to follow ; it deals with the facts of existence but not with the values of existence. It gives some light to them that sit in darkness, but it has little consolation for those in the shadow of death and it does not guide out feet into the way of peace. Even if science should make large scale fighting wars impossible it can do little against the more subtle wars by infiltration. It offers us great possessions but as the old aristocracy knew, great possessions imply great personal responsibilities. Democracies still have this to learn. That is one of our greatest problems to-day.

But there is no need for pessimism. Looking back to the time of our first meeting here, one can find much ground for hope. Those, too, were days of terrible anxiety and strain, and yet there were then born many movements and activities which, hardly noticed at first, blossomed later into full vigour, and greatly enriched our national life. How many important centenaries have been celebrated in the past few years ! And it may be that, unperceived by us, new movements are now starting into life equally full of momentous possibilities for the future. Science can help us best if we have a sustaining faith, a high purpose in life and unflinching courage to pursue it.

SECTION A—PHYSICS AND MATHEMATICS

SOME DEVELOPMENTS IN ASTRONOMICAL INSTRUMENTS

ADDRESS BY
SIR HAROLD SPENCER JONES, F.R.S.
PRESIDENT OF THE SECTION

UNTIL the eighteenth century was far advanced, astronomical observation was concerned almost entirely with the determination of the positions of the Sun, Moon, planets and stars. For more than a century after Newton had formulated the principle of universal gravitation the main interest in astronomy was centred in the application of this principle to the detailed explanation of the motions of the bodies in the solar system. The results of the application of the first primitive telescopes to the study of the heavens had been important and exciting: the discoveries of spots on the Sun, of the Sun's rotation, of mountains and other formations on the Moon, of the phases of Venus, of the four major satellites of Jupiter and of their revolution round Jupiter. Further discoveries were slow in coming; Christiaan Huygens recognised the ring structure of Saturn's appendages and discovered Titan, its brightest satellite; J. D. Cassini discovered the rotation of Mars, the division in Saturn's ring system which bears his name, and between 1671 and 1684 four satellites of Saturn. But there, for almost a century, the discoveries ended. Because of the large aberrations of the simple objectives, greater and greater focal lengths were required as apertures were increased, so that telescopes became unwieldly and awkward to manipulate. The long focus telescope consequently soon fell into disuse.

The improvements in the time keeping of clocks which followed from the application of the pendulum by Galileo and Huygens and the invention of the anchor escapement by Hooke greatly simplified the determination of the positions of the heavenly bodies. The method which had been used by Tycho Brahe was first to measure the meridian altitudes of the stars; assuming the latitude of the place of observation to be known, these measures determined their declinations. The distances apart of pairs of stars were then measured with a sector. The declinations of the stars being known, the differences in right ascensions of each pair of stars could be computed. To derive absolute right ascensions, reference to the Sun was necessary. For this purpose Venus was used as an intermediary. The measurement of the distance of Venus from the Sun gave the right ascension of Venus. By measuring the distances of Venus from several zodiacal stars, the right ascensions of these stars were obtained. The differences of right ascensions of pairs of stars already derived then enabled a system of right ascensions to be built up. When pendulum clocks became available, this involved method for determining right ascensions was replaced by the observation of the sidereal times of transit across the meridian, the sidereal time of meridian transit of a body being equal to its right ascension. The observations were usually made with a mural quadrant, fixed to a wall as nearly as possible in the meridian, the telescope being pivoted at the centre of the quadrant, which was graduated.

The Danish astronomer, Römer, realised that it was much better to support the telescope by two pivots at the end of an axis than by a single short pivot. The *machina domestica* which he constructed in 1689 was the first transit instrument. It was provided with conical pivots to avoid shake and with a counterpoise weight-relieving system, while the graduations of the circle were read with a microscope. The first transit circle to be used at the Greenwich Observatory was installed by Halley in 1722; it had cylindrical pivots resting in V bearings, which were adjustable in azimuth; a hanging level was employed for determining the error of level of the axis. Though a small instrument, its design was a considerable improvement upon that of Römer.

The transit instrument soon came into general use as the standard instrument for the observations of transits. The mural quadrant developed into the mural circle, provided with a complete graduated circle; the axis was lengthened and fitted with two

bearings and with a weight-relieving system ; it was used for the determination of declinations. During the nineteenth century the mural circle gradually fell into disuse. A graduated circle and a system of microscopes for reading the graduations were added to the transit instrument which, in its new form of the transit circle, became the standard instrument for the determination of both right ascensions and declinations. It has continued to be used for this purpose up to the present day ; improvements in detail have been made to give increased stiffness to the axis, to provide better stability for the microscopes, to minimise the effects of temperature variations, to enable adjustments to level and azimuth to be more conveniently and accurately made, to enable variations of azimuth to be more precisely controlled, and to minimise the personal equations of the observers.

Meridian observations form one of the few branches of astronomy in which it has not yet proved possible to replace visual by photographic observations. Photography has been called in, however, to reduce the amount of visual observing that is required. With wide-angle-lens cameras of suitable design, star fields measuring $10° \times 10°$ can be photographed and sharp images obtained over the whole field. The positions of two or three dozen of the brighter stars, well distributed over the field, are determined with the transit circle and are then used as reference points for the determination of the positions of all the other stars on the plate. On the other hand, there has been a progressive increase in the accuracy that is required. The limitations of the transit circle then become important.

In order that the telescope should move in the meridian, the axis of rotation should be horizontal and should point exactly east and west, while the optical axis of the telescope should be perpendicular to it. These ideal conditions are not obtainable, so that the errors of level, of azimuth, and of collimation must be determined. During observations the telescope is exposed to the open air, often with a wind blowing and with the temperature changing : the temperature conditions of the instrument are therefore likely to be non-uniform and variable. The errors of adjustment are extremely sensitive to these temperature conditions ; they may change rapidly so that the accurate control of the changes becomes a matter of great difficulty. The flexure of the axis may not be symmetrical with respect to the telescope, causing the axis of the telescope to be twisted by an amount which may vary with the position of the telescope. The bearings at the ends of the axis are never exactly cylindrical and may not be in absolute alignment : the determination of the figures of the pivots is a matter of some difficulty. Finally, as the telescope is turned into all sorts of directions, there is the possibility of variable errors being introduced by mechanical imperfections in the design and construction of the micrometer eye-end or of the mounting of the objective.

It is not surprising, when the many ways in which the transit circle can deviate from the ideal instrument are borne in mind, that systematic differences between the results given by different instruments are found. Systematic differences can occur even between the results given by the same instrument in different conditions of use, such as by reversing the instrument in its bearings or by interchanging the objective and the micrometer eye-end. These obscure systematic effects limit the accuracy with which a fundamental reference system of star places can be provided. Much consideration is therefore being given to other designs of meridian instrument, which are likely to be less affected by systematic errors.

Many of the difficulties inherent in the conventional transit-circle can be eliminated if the telescope is fixed and the movable part is restricted to a mirror for reflecting light into the telescope. The mirror is mounted with its plane parallel to its horizontal east-west rotation axis and its position is determined, as in the transit circle, by means of graduated circles and microscopes ; stars are observed by reflection from the mirror in one or other of two fixed telescopes situated in the meridian. Each of these telescopes is fitted with a motor-driven micrometer eyepiece and, being fixed, can be thoroughly insulated against temperature changes. They can also conveniently have a longer focal length than is practicable for a moving telescope, with the advantage of a larger scale. The micrometers being in a fixed position, the effects of any mechanical imperfections are much reduced. The two telescopes serve not only for observations of the stars but also as collimators, collimation being deduced by autocollimating each telescope in turn on the mirror and then sighting one telescope on the other with the mirror lifted out of position. Level error and nadir point are determined with the aid of a mercury horizon below the mirror, the normal to the mirror being inclined at 45° from the vertical downwards. Flexure effects are much reduced because the moving parts are relatively light. By mounting both telescopes as well as the short piers for supporting the mirror axis on one massive concrete bed, the errors of adjustment are made much more stable. The method has never yet been tried out in practice, though a

model has been made at Greenwich as a preliminary to a detailed design, while the theory of the instrument has been developed.

Meridian instruments are used for the determination of time as well as of position. The determination of time being normally based upon the brighter equatorial stars, whose positions are well determined, a small telescope can be used, with the advantage of being capable of quick reversal in its bearings. By reversing the telescope in the middle of the observation of the transit of a star the collimation error is eliminated. The advantage of this more than compensates for the small scale of the telescope. The variable errors of level and azimuth remain, however, and impose a limit on the accuracy of the observations.

An interesting modification of the transit instrument, in which the instrumental meridian is defined by purely optical methods is due to Danjon. The pivots accordingly do not require to be figured to a high accuracy ; they serve merely as a support for the instrument and no corrections for pivot error are required. The essential part of the instrument consists of an accurately worked Wollaston reversing prism, whose long face defines the instrumental meridian. The prism is placed in front of the objective, covering the central portion of it and leaving the two outer parts, in the form of segments of a circle, uncovered. The prism and the telescope can be turned about an east-west axis, which passes centrally through the prism. When pointed at a star near meridian passage, two images are seen, one of which is formed by light that has passed through the prism, and the other by light that passed through the two outer segments of the lens, the beams that produce the two images having the same principal ray. The two images move in opposite directions and at the moment of meridian transit (assuming that there are no errors of adjustment) the two images coincide. The direct image is observed before coincidence with an impersonal micrometer, as in an ordinary transit observation, and the reflected image is observed after coincidence over the same portion of the field. The mean of the two series is used to derive the instant of coincidence.

The long face of the prism is silvered and a collimating telescope is mounted on the axis of rotation, in an east-west direction. This telescope has a micrometer provided with two screws in directions at right angles. The coincidence of the wires with their images determines the normal to the mirror face of the prism. An essential part of the observation of each transit is the determination of this normal. The azimuth of the auto-collimating telescope is controlled by a distant azimuth mark. The telescope is given a compact form by deflecting the light, after its passage through the objective, by a plane mirror, inclined at an angle of 15° to the optical axis, and then by an equilateral prism, placed on the axis of rotation, into the eye-piece, which is on the axis of rotation, and therefore always in a fixed position. A telescope of this design is in use at the Paris Observatory and determines time appreciably more accurately than the small transit instruments generally used.

The most precise determinations of time are made, however, in an entirely different manner, by an adaptation of the principle used by Airy in his Reflex Zenith Tube. The telescope is fixed and points vertically to the zenith. The length of the tube is approximately half the focal length of the objective, the converging light being reflected by a mercury pool, whose position is adjusted so that the light is brought to a focus in the horizontal plane through the second Gaussian point of the lens. When this condition is satisfied the observations are not affected by any error of level of the telescope. A photographic plate is placed in this plane and an image of a near zenith star is obtained by giving a short exposure of, say, 20 seconds, the plate being travelled along at the appropriate speed. The objective and plate-holder are then turned through 180° and a second similar exposure is given, the two exposures being arranged to be as nearly as possible symmetrical about the instant of meridian transit. The times of both exposures are chronographically recorded. The two images of the star on the photographic plate are at nearly the same distance east or west of the meridian ; by measuring the small relative displacement in the east-west direction, the time of meridian transit can be easily deduced. The stars observed with such an instrument lie in a narrow belt of declination and are consequently faint : their positions must be derived from meridian observations, but the zenith tube observations rapidly smooth out those positions. All the troubles inherent in the transit instrument are eliminated : errors of level are immaterial, errors of azimuth do not affect observations at the zenith, collimation error does not enter ; personal equations are avoided by the use of photography ; the instrument is fixed, so that it can be well protected from the effects of temperature change; it points to the zenith where atmospheric refraction effects are a minimum. In addition, a longer focal length can be used, with a correspondingly greater scale than is possible for a transit circle. The accuracy attainable with this type of instrument is very high.

As long as the astronomy of position provided the main interest, meridian telescopes were the most important instruments in observatories. But the development of physical astronomy has caused them to be superseded almost entirely by equatorial telescopes, both refractors and reflectors, except in the principal national observatories.

Newton discovered that the dispersion of light was the principal cause of the aberrations of simple lenses. But his mistaken ideas about the dispersions of different glasses caused him to conclude that it was impossible to construct achromatic lenses and that it was necessary to use mirrors. He himself made a mirror of bell metal, using a pitch polisher for polishing it, and hit upon the simple method of getting the image outside the tube by means of a small plane mirror, placed a little inside focus and at an inclination of 45° to the axis of the telescope—the so-called Newtonian reflector. But the difficulties of working, figuring and polishing metal mirrors prevented further development for some fifty years. It was not until 1720 that a satisfactory reflecting telescope of the Newtonian type was made ; this was the achievement of John Hadley, who successfully took the essential step of parabolising the mirror, which had never previously been achieved and who developed methods of testing for the parabolical figure. Molyneux, Hawksbee, and in particular James Short developed Hadley's methols. Short is said to have been possessed of a craftsmanship that approached wizardry ; he not only fully mastered the art of figuring the paraboloid but also took up with success the Gregorian construction, with its ellipsoidal mirror. His speculum metal mirrors were of great relative aperture and the composition of the metal was such that they were free from tarnish and maintained their polish and good definition. Short was very secretive about the methods he used : he never revealed them and before his death he even destroyed his tools.

William Herschel in the latter part of the eighteenth century took up anew the making of reflecting telescopes, learning by hard experience the difficult technique of figuring and polishing. His two most famous telescopes were the 20-foot telescope of 19-inch aperture and the great 40-foot telescope of 48-inch aperture. The mounting of these telescopes was of the altazimuth type ; the telescope was suspended between two wooden frameworks and was raised or lowered in altitude by means of ropes and pulleys, while the whole mounting could be turned in azimuth. The many important discoveries which he made are a tribute not only to the quality of his optical work but also to his skill and assiduity as an observer.

Speculum metal is a hard brittle alloy of copper and tin containing about one part by weight of tin to two of copper. It will take a high degree of polish and has a moderately high reflecting power, which drops from about 70 per cent. in the red to 50 per cent. in the violet. The metal is difficult to cast and anneal and William Herschel was able successfully to cast his larger disks only by increasing the copper content to 75 per cent. Though a beautiful polish was obtainable, the surface tarnished rapidly. The 48-inch mirror had to be repolished, which meant also refiguring, every two years. When Sir John Herschel took the 20-foot telescope to the Cape, he took with him three specula and the polishing machine. In the Cape climate he found that a mirror required repolishing after being in use for two or three months. The need for frequent repolishing and refiguring prevented reflecting telescopes with speculum mirrors from coming into more general use.

Though the altazimuth type of mounting was convenient for small portable instruments it was inconvenient for large fixed instruments, as the telescope had to be continually moved in both co-ordinates to keep an object in the field of view. The equatorial mounting, in which one of the axes about which the telescope is turned is parallel to the earth's axis, has the great advantage that, to keep an object in the field of view, it is necessary to turn only about this axis—the polar axis. The idea was known to Hooke who, in 1674, proposed the equatorial mounting for a quadrant, embodying the suggestion of controlling the diurnal motion by clockwork. The first telescope to be provided with an equatorial mounting appears to have been Römer's *machina aequatoria*, constructed in 1690. In 1760 Sisson constructed an equatorial sector with a telescope, this being essentially the simple English mounting in which the polar axis is supported at each end, the telescope being carried by a cross axis to one side of the polar axis. The English yoke mounting, in which the cross axis is supported in the yoke which forms the polar axis, is due to Ramsden who in 1791 mounted a telescope 5½ feet long in this way. The new form of mounting was widely adopted by English constructors for small telescopes, but the problems of mounting large telescopes equatorially were not yet solved.

Herschel's 40-foot telescope was surpassed in size by the large telescope built by Lord Rosse for his private observatory at Birr Castle. He endeavoured to avoid the difficulties of casting large disks of speculum metal by fabricating mirrors of cellular form ; the reflecting face was supported by radial ribs, with transverse stiffening pieces,

resting on a base which formed the back of the mirror. Thus the interior was divided into compartments through which the air could circulate freely and so ensure temperature equilibrium. But satisfactory images could not be obtained, as the surface yielded between the various supports during figuring. He therefore reverted to solid disks and made great improvements to the technique of casting and figuring specula. His large reflector, completed in 1845, had an aperture of 72-inches and a focal length of 54 feet. The motion of the telescope was limited to about one hour on either side of the meridian, two solid walls of masonry being used for its support. More than sixty years were to elapse before a telescope of greater light gathering power was constructed.

With increase in size of the mirror greater care must be taken in the method of its support, so as to avoid distortion of its figure. Sir John Herschel states that on one occasion he ' supported a mirror simply against a flat board, at about 45° elevation from the horizon.' In this state its performance was tolerably good ; but on stretching a thin packthread vertically down the middle of the board, so as to bring the weight to rest on this as on one axis, the images of stars were elongated, in a horizontal direction, to a preposterous extent, and all distinct vision utterly destroyed by the division of the mirror into two lobes, each retaining something of its parabolic figure, separated by a vertical band, in a state of distortion, and of no figure at all.' He used to rest the mirror on six or eight thicknesses of coarse woollen baize, or blanketing, with two supports at the rim of similar material, many times doubled, occupying about 30° each of the circumference, leaving between them an arc of some 40° opposite the point which was intended to be placed lowermost in the tube. Lord Rosse supported his 72-inch mirror on a system of self equilibrating triangular supports having a bearing pad at each corner, a method which proved to be completely satisfactory.

Though the achromatic lens had been invented by Chester Moor Hall in 1733 and patented in 1758 by John Dollond, who acquired great renown for the excellent quality of his objectives, the size was limited by the poor quality of optical glass, which was far from homogeneous, the flint particularly being full of faults. A 3-inch objective was at that time considered rather large. The development of the modern refracting telescope was made possible by the collaboration between Fraunhofer and the Swiss artisan, Pierre Louis Guinand. Being interested in the construction of telescopes, Guinand, living near Neuchâtel, imported some optical glass from England. Finding the quality to be poor, he set out to make better glass for himself. Despite early failures he persevered and by 1799 was making excellent flint disks up to 6 inches diameter. In 1805 Fraunhofer persuaded him to move to Munich and their collaboration made both the German optical glass industry and the modern refractor. Fraunhofer devised methods of working glass with mathematical precision on automatic machines and the science of computing objectives well-corrected for the aberrations. He perfected the equatorial mounting and developed the clockwork drive.

Because of these improvements and because also of the difficulties of casting and figuring specula, the refractor for a time almost completely superseded the reflector. In 1828 Lord Rosse wrote, ' Many practical men whom I have spoken to seem to think that since Fraunhofer's discoveries the refractor has entirely superseded the reflector and that all attempts to improve the latter instrument are useless.' Larger and larger objectives were made by Merz and Mahler, the successors of Fraunhofer in Germany ; by Cauchoix in France ; by Grubb and Cooke in England ; and by Alvan Clark in America. The largest objective in 1824 was the 9½-inch made by Fraunhofer for Dorpat ; in 1839, the 15-inch, made by Merz and Mahler for Poulkova ; in 1871, the 24½-inch made by Cooke for Newall, now at Cambridge ; in 1878 the 26-inch made by Grubb for Vienna ; culminating in 1888 with the 36-inch by Alvan Clark, for the Lick Observatory, and in 1897 with the 40-inch, also by Alvan Clark, for Yerkes Observatory. No larger objectives have ever been made, nor are they likely to be made in the future. The long-focus refractor is valuable for certain specific purposes. It gives good images over a flat field of moderate extent. For a focal length of 25 feet the useful field is about one degree and varies approximately in the inverse proportion of the focal length. As the focus is not subject to rapid changes with temperature, the refractor is a good instrument for precise and accurate measurement, either by visual or photographic methods. Its most important contribution to astronomy has been in the determination of stellar distances ; it is also well suited for the measurement of close double stars. But the refractor has its limitations. In order to keep the optical aberrations reasonably small, increase in aperture necessitates a corresponding increase in focal length. The Lick 36-inch refractor has a focal length of 58 feet ; the Yerkes 40-inch refractor has a focal length of 63½ feet. Such telescopes tend to be unwieldly in use, and require a dome of large size. The out-of-focus light, the so-called

secondary spectrum, is troublesome for many purposes and makes the refractor far from ideal for use with a spectrograph. The aberrations could be reduced by using a more complicated lens system, but complicated lens systems of large aperture have never been constructed. The aperture-ratios of large refractors are unsuitable for photographing faint extended objects. Moreover, it is difficult to obtain disks of optical glass of great size which are free from bubbles, strains, striations, veins, and other troublesome defects. The refracting telescope has reached a practical limit in size and aperture. For greater light-gathering power astronomers have necessarily been compelled to revert to the reflecting telescope.

The development of the silver-on-glass mirror, by avoiding the difficulties inherent in specula, began to bring the reflecting telescope back into favour in the seventies and eighties of the last century. Lassell's 47-inch reflector, made in 1860, was the last large reflector with a speculum mirror to have been used, as far as I have been able to ascertain ; the 48-inch speculum reflector made in 1867 by Grubb for Melbourne was never used. The idea of making mirrors of glass was not new : Short in 1734 had made some glass mirrors, silvered on the back, which Maclaurin declared to be excellent. In 1827 Airy had proposed that a layer of silver should be deposited on the surface of a figured glass disk. But the method did not become practicable until Liebig discovered a simple chemical process for depositing a thin film of silver on a glass surface. The methods of figuring and testing glass disks were due primarily to Foucault who, in 1857, presented his first telescope with a silvered mirror to the French Academy. His knife-edge test is still a valuable tool to the optician. Further developments in technique were made by Draper in America, by Common in England, and by Ritchey in America.

In recent years aluminium has almost entirely replaced silver as the reflecting coating on a figured glass surface. The reflectivity of a freshly deposited silver film exceeds 90 per cent. throughout the visible spectrum but on the short wavelength side of 4500A it falls at first slowly and then rapidly to a minimum value at about 3200A ; a silver film is practically transparent to light of this wavelength. An aluminium film, on the other hand, has a reflectivity which is nearly as high as that of freshly deposited silver in the visual region and does not show the severe drop in the ultra-violet. Between 3250A and the limit of atmospheric transmission at about 2900A the aluminium film has therefore very marked advantages over silver for astronomical investigations. The film does not tarnish, adheres strongly to the glass surface, and is hard, so that it can be cleaned. Under favourable conditions it will last for several years with very slight decrease in reflectivity. The technique of depositing the aluminium film, by a process of vacuum distillation at a pressure of about 10^{-5} mm. of mercury, is due to Strong.

No material has yet been found which is more suitable than glass as a medium for supporting the reflecting film. Glass is capable of taking a high polish and is free from any tendency to warping or distortion with age. But its relatively large coefficient of expansion and poor thermal conductivity are disadvantages. Assuming the temperature to be uniform throughout the disk, the position of the focus of the mirror will depend upon the temperature ; for a given change of temperature, the displacement of the focus will be greater the larger the coefficient of expansion. If the temperature is not uniform throughout the disk, distortion of the figure of its surface will occur, which will impair the quality of the images ; the effect will be greater the thicker the disk and the lower its thermal diffusivity. Pyrex glass is now used in preference to plate glass because of its smaller coefficient of expansion and its larger thermal diffusivity. The thinner the disk the more rapidly it will reach a state of temperature equilibrium, but then also the more flexible it is, and the more elaborate the support system must be. The requirements are therefore to some extent self contradictory and the choice of thickness is a matter of compromise.

Sagging of the disk under its own weight with consequent distortion of the figure of the reflecting surface can be counteracted by a properly designed support system. It is usual to provide three fixed supports, which serve to define the position of the disk, and a number of floating supports of the counterpoised lever type. For a large disk it is necessary to distribute these supports around three rings ; the number of supports in each ring must be sufficient to keep the amount of festooning between adjacent supports within safe limits. Couder has developed criteria, based partly on dimensional considerations and partly on experimental results, from which the number of supports required in each ring for a glass disk of specified diameter and thickness can be evaluated. For satisfactory performance it is necessary that the pressure of each support should compensate for the appropriate portion of the weight of the disk correct to about one-tenth of one per cent. in all positions of the telescope. Careful design of the counterpoise system in order to minimise friction and critical adjustment of the position of the counterpoise on

each lever arm are therefore necessary. The edge support is usually provided by an edge-band, which is also carried by a counter-poised lever system.

For the 200-inch Hale telescope it was decided to reduce the weight of the disk by constructing it with a thin face supported on a honeycomb structure, consisting of 36 cylindrical pockets, lying on five concentric circles, each pocket being connected to each of the adjacent pockets by a glass rib, thereby providing stiffness. These pockets were designed to form part of the support system. Each was ground accurately cylindrical and fitted with a steel sleeve, gaskets being inserted between the bearing faces and the glass. Each sleeve is connected to the cell-frame by an integral two-component system of balance weights and levers. Double gimbal bearings on the steel sleeve and on the support fixed to the cell frame prevent constraints. Three of the lever systems are tied down by circumferential springs to constrain the disk against rotation about the axis, while three others are tied down as axial defining points. A central tube passes through the 40-inch diameter central hole in the mirror : four centre radial jacks, made of invar and steel to compensate for expansion of the central steel tube, define the radial location of the mirror, no edge band being required. These centre jacks and 12 edge radial squeeze arcs, to remove gravitational astigmatism, are provided with ball-bearing faces to allow freedom in the plane normal to the applied reaction. A number of fans have been mounted in the back of the mirror cell to draw air from the front to the back of the mirror in order to secure better temperature equilibrium.

The conventional large reflector has a paraboloidal mirror, with an aperture-ratio usually of from f/5 to f/3·3. It can be used at the prime focus, either directly or in the Newtonian form, or in various Cassegrain or Gregorian forms (the former being usually preferred because it does not require so long a tube) with considerably longer equivalent focal lengths. The secondary focus is then conveniently either just below the main mirror or, by appropriate plane mirror reflections, the beam can be thrown down a hollow polar axis to a fixed Coudé focus, where a large spectrograph with high dispersion can be used. The Cassegrain combination usually works at about f/15 and the Coudé combination at about f/30.

The parabolic reflector is free from chromatic and spherical aberration, but at small distances from the axis the images are badly affected by coma, the amount of which is proportional to the distance from the axis and to the square of the ratio of aperture to focal length, and by astigmatism, the amount of which is proportional to the square of the distance from the axis and to the ratio of aperture to focal length. These defects are immaterial for spectrographic observations, for which axial images are used, or for photo-electric observations ; but they impose a serious limitation on direct photography. In order to overcome these defects and to enlarge the usable field, Ross has designed special correcting lenses of zero power to be placed in the converging beam near the focus. The corrector has negative coma equal to the positive coma of the paraboloid, but the corrections of coma and of astigmatism are only obtained at the expense of introducing spherical aberration. For the 200-inch telescope two correcting lenses have been designed. One is intended for use under the best conditions of seeing and will have a spherical aberration confusion disk of 1·5 seconds diameter ; it will increase the area of the field of good definition tenfold. The other is for use under average conditions of seeing and will have a spherical aberration confusion disk of 2·5 seconds diameter ; it will increase the area of good definition some twenty- or thirty-fold. The correction of the spherical aberration is obtained, however, at the expense of introducing rather heavy distortion.

Various modified designs of reflecting telescope have been devised to reduce the effects of coma. Foremost amongst these is the Schmidt telescope, designed by Bernard Schmidt in 1930. Schmidt used the fact that a spherical mirror has radial symmetry. If, therefore, a diaphragm is placed at the centre of curvature of a spherical mirror, any parallel bundle of rays will be focused into a circle of confusion about half-way between the centre of curvature and the mirror. The spherical aberration is constant over the whole field. For an aperture ratio of f/10 and at the best focus it amounts only to 1·6 seconds ; the paraxial images are then better than those obtained with a paraboloid at any distance greater than 30 minutes from the axis. But as spherical aberration increases with the cube of the ratio of aperture to focal length, for a spherical mirror working at f/3 it amounts to 60 seconds. Schmidt remarked that if a thin glass plate were placed in contact with a spherical mirror, the process of parabolising could be replaced by giving a slight variation to the thickness of the glass plate. This would not result, of course, in any improvement to the optical performance. If, however, the glass plate is placed at the centre of curvature and deformed aspherically by figuring in the right way, the same result is achieved, but the mirror is, in effect, parabolized in every direction at once,

because of the radial symmetry. At 10° off the axis, the effect on the wave front of the inclination of the rays to the plate is only of the order of one interference fringe. A large field of sharp focus is then obtained, free from the defects of spherical aberration, coma, and astigmatism. The focal surface lies on a sphere whose radius is the focal length of the system, and the photographic plate or film must be constrained to fit this surface exactly. Some chromatic aberration is introduced if the correcting plate is made of a single variety of glass ; if full correction is obtained in the yellow, there will be over-correction in the blue and under-correction in the red. The colour error is small, however, except at extreme aperture-ratios such as f/0·6, being inversely proportional, for a given aperture, to the cube of the focal length. In such cases it can be eliminated, if desired, by constructing a paired correcting plate of crown and flint. Because the correcting plate is so nearly flat, Schmidt telescopes can be used at very large aperture-ratios. At f/1 a useful field of 25° can be obtained. In order that the images at the edge of the plate may be formed by beams which are not partially cut off, the diameter of the mirror must exceed that of the free aperture by twice the diameter of the photographic plate. Thus a 24–36-inch Schmidt telescope is one which has a 36-inch diameter spherical mirror with a 24-inch diameter correcting plate : the photographic plate can have a diameter of 6 inches without vignetting.

The Schmidt telescope is usually employed in astronomy at an aperture-ratio of from f/3 to f/5. It has the advantages of high speed and of a large field of good definition ; the images are, moreover, good under conditions of relatively poor seeing. It is an ideal telescope for stellar photometry, for the study of stellar distribution and, in conjunction with an objective prism, for classification of spectral types of stars. Its chief defect is that the tube length must be twice that of a parabolic telescope of equal focal length, but this is to some extent compensated by the fact that the paraboloidal mirror would be used at a smaller aperture-ratio.

Various modifications of the Schmidt design have been proposed. Mention will be made of only one of these—the Schmidt-Cassegrain systems, which have been discussed by Linfoot. Such a system consists of a concave primary mirror, usually with a central hole, a convex secondary mirror, and an aspherical correcting plate through which the light passes on its way to the primary. After reflection at the primary and secondary mirrors the light comes to a focus near the pole of the primary. There is a whole range of these systems, in some of which both mirrors are spherical and in others one or both mirrors are figured. Those with spherical mirrors are the easiest to make and are of the greatest interest. In the monocentric type the primary and secondary mirrors are concentric spheres and the correcting plate passes through their common centre of curvature. The field is curved, being concentric with the mirrors. The system is anastigmatic, gives distortionless imaging on its image sphere, and is free from first order spherical aberration and coma. Another group with spherical mirrors has the advantage of a flat field ; the off-axis astigmatism is so small as to cause no observable effect on the image size. For a system of given aperture and overall tube length, the diameter of the secondary depends upon the distance between the mirrors ; a convenient system is one in which the diameter of the secondary is about one-third that of the primary. The Schmidt-Cassegrain systems have the advantage that, when the image is formed in a convenient position near the pole of the primary, the overall length is roughly equal to the focal length, instead of twice the focal length as it is for a straight Schmidt ; the figuring required by the correcting plates is, on the other hand, much heavier than for the straight Schmidt. The systems are very highly corrected, the most important residual error being high-order coma, due to lack of exact satisfaction of the sine condition.

All large reflectors that have been constructed have paraboloidal mirrors. The question arises whether a large telescope can be constructed with a spherical primary mirror to serve the dual purposes both of direct photography with a large field, of good definition and also of spectroscopy with a long equivalent focal length, permitting a high dispersion spectrograph to be used. This is theoretically possible either with a straight Schmidt system or with the Schmidt-Cassegrain systems by dismounting the correcting plate and inserting either a hyperbolic convex Cassegrain secondary or an elliptic concave Gregorian secondary. In either case the secondary mirrors need to be deeply aspherical ; the Gregorian arrangement can be tested more easily, but the Cassegrain arrangement requires a smaller diameter mirror and therefore gives less obstruction and has the further advantage that the field curvature of the secondary is partly cancelled. Two different secondaries could be provided, if considered advantageous, one to give a beam at about f/15 or f/20 for observations at the Cassegrain focus and the other to give a beam at about f/30 or f/40 for observations at the Coudé focus with an underground spectrograph. With either arrangement the

off-axis coma is appreciably larger than with the conventional paraboloidal primary, reaching the Rayleigh limit at a few seconds of arc from the axis. Though this is immaterial for the observation of an object on the axis which is sufficiently bright for its image on the slit of the spectrograph to be used for guiding, it would give rise to difficulties when the object to be observed is so faint that some other star in the field must be used for guiding. A further difficulty is that the tolerance for the lateral alignment of the secondary mirror with respect to the primary, which is necessary to prevent comatic image spread, is so small that special care would be required in the mechanical design. Systems of both types have been under consideration for the Isaac Newton reflector.

Several different methods of mounting an equatorial telescope have been used. In the German mounting the declination axis is carried at the end of the polar axis, just beyond its upper bearing; the telescope is mounted at one end of the declination axis and counterposed by a weight at the other end. The great Lick and Yerkes refractors have this type of mounting, but it has not been used for any large reflector. The simple English mounting is somewhat similar, differing in that the polar axis extends beyond the cross declination axis, and has its upper end supported by a bearing mounted on a pier. The Victoria 72-inch and the Toronto and Radcliffe 74-inch reflectors are mounted in this way. In the English yoke mounting, the polar axis, which is supported in bearings at its upper and lower ends, is in the form of a yoke, the telescope being carried on a short declination axis mounted in the yoke. The Mount Wilson 100-inch reflector has this form of mounting; it suffers from the disadvantage that the telescope cannot be used on a portion of the sky around the pole. A modified form, which is free from this objection, was adopted for the Mount Palomar 200-inch reflector; the upper bearing is in the form of a stiff open horseshoe-shaped girder, 4 feet wide and 46 feet in outside diameter, enabling the telescope to lie in the throat of the horseshoe, pointing directly to the north pole. In the fork type of mounting, the polar axis is short and carries at its upper end a short stiff fork, in which the telescope is hung. The Harvard and Mount Wilson 60-inch reflectors have this mounting, which is also to be used for the proposed Lick 120-inch reflector. It is in many respects the most convenient form of mounting but, because of the considerable overhang beyond the upper bearing of the polar axis, particularly when designed to permit a spectrograph to be used at the Cassegrain focus beneath the main mirror, the fork must be made of immense strength to avoid troublesome flexures.

For observations at the Coudé focus, when the light is thrown down the hollow polar axis by reflections at plane mirrors, the simple English mounting has the advantage that the same method can be used for observations of objects at all declinations. With the yoke and fork mountings it is not possible to use the same method for both high and low declinations, which introduces some complications in design.

In the Mount Wilson 60-inch and 100-inch reflectors mercury flotation systems are used to relieve friction. The method is not a convenient one and ball or roller bearings have been generally used for more modern telescopes. For the 200-inch telescope oil-pad pressure bearings were adopted for supporting the great horseshoe at the upper end of the polar axis and for the hemispherical thrust bearing at its lower end; oil at a pressure of 250 lbs. is forced through a central hole in each of the bearings, which are about 28 inches square. The telescope thus floats on a film of oil and a very small and uniform torque is required to turn it. This method is ideal for large telescopes and is likely to become standard practice.

Of the various instruments which have been developed in recent years for special purposes, reference can be made only to the coronagraph which has made it possible to observe and photograph the corona at times when the Sun is not in total eclipse and which has therefore become a powerful tool in solar research. The total brightness of the corona is only about one-millionth of that of the Sun. Under ordinary conditions the amount of sunlight scattered by dust and other particles in the atmosphere is so much greater than the light of the corona that the corona is completely blotted out. On a clear day when the sky appears of a deep blue colour it normally has a milky appearance near the Sun, due to this scattered light. The corona has an emission bright-line spectrum. Eclipse observations have provided determinations of the absolute brightness of the corona in its various radiations and it can be concluded that the light of the corona in any of its principal emissions is brighter than skylight per Angstrom unit of spectrum when the air is free from dust and of sufficient rarity, so that the atmospheric scattering is entirely molecular. This provided Lyot with the practical basis for the design of an instrument in which an artificial eclipse of the Sun is produced and the corona is made visible.

In Lyot's coronagraph, the objective forms an image of the Sun in the focal plane, where an occulting disk, very slightly larger than the

Sun's image, is placed. A slightly tilted flat mirror in front of the disk reflects most of the sunlight to one side, where it is trapped. Behind the disk is placed a field lens which forms an image of the objective on a camera lens, whose diameter is slightly smaller than the image, so that the light which is diffracted at the edge of the objective is cut out. The camera lens forms an image of the first image of the Sun on a photographic plate. Any residual scattered light is trapped by various circular diaphragms. The optical components are made with great care from selected glass, entirely free from striae and bubbles, and are given the greatest perfection of polish, because minute scratches would increase the scattered light. The coronagraph must be placed, for successful observation, at an attitude which is sufficiently high to be above the dust layer.

This survey of some of the developments in astronomical instruments has necessarily been far from complete. No reference to details of design and construction, to methods of guiding and control, to the many techniques used in observation, nor to the design of spectrographs and other ancillary equipment has been possible. These matters are of great importance for the efficient performance and use of a telescope and each presents many problems of interest. Nor has it been possible to refer to a variety of instruments which have been designed and constructed for specific special programmes of observation. The design of almost every astronomical instrument in fact presents special problems ; certain general broad principles may be followed, but in its details the design must be conditioned by the programmes of observation on which the telescope is to be employed. It is by the closest collaboration between the user, the designer, and the constructor that progress in observational astronomy can best be made.

SECTION B.—CHEMISTRY

THE INFLUENCE OF THE STUDY OF COMBUSTION ON THE PROGRESS OF CHEMISTRY

ADDRESS BY
SIR ALFRED EGERTON, F.R.S.

IT is a great honour to be invited to preside over this section of the Association's meeting in this ancient city, Newcastle. I became still more sensible of the honour and of the responsibility on scanning the records of the previous meetings. How interesting that record is !

The first meeting of the Association in Newcastle was in 1838. Section B was then called the Chemistry and Mineralogy Section ; William Whewell presided and Daubeny, Graham, Miller, Pattinson and Thomas Thomson were with him. Whewell, author of a treatise on Mechanics, had reformed the teaching of mathematics at Cambridge ; he was Professor of Mineralogy and later of Moral Philosophy, and Master of Trinity. Of Whewell, Sidney Smith wrote, rather cruelly, ' Science was his forte and omniscience his foible.'

The reports of those early meetings indicate that the British Association was conscious of its true mission. Murchison, the Treasurer, seems to have relieved the President, the Duke of Northumberland, of the task of making the address, and in it he specifies the purpose of the Association . . . ' not like articles in encyclopedia to teach and diffuse science, but to advance it, to show what has been done with a specific view to what there remains to do ' . . . ' to reconnoitre debateable land ' . . . and point ' to conquests to come.' Murchison referred to the mutual importance of the study of crystal form to both mineralogy and to chemistry. If he could have penetrated the future with an X-ray mind, ' the conquests to come ' would have surprised him. The researches at the Cavendish Laboratory, Cambridge, on the structure of giant globular protein molecules are a recent striking example of the great achievements in this crystallographic field.

There is no address from Dr. Whewell in the records of the meeting : the Section does not appear to have had to listen to its President's omniscience. The Section had in hand investigations on the effect of sea and rain water on cast and wrought iron, and experiments on the action of heat on inorganic substances ; these are perennial subjects. One must recall that chemistry was then still in the adolescent stage ; it was three years later that the Chemical Society was founded, with the illustrious Graham as its President, one of the Vice-Presidents of the Section at this meeting in Newcastle. You will remember that Liebig after a visit to England about this time was critical of the state of chemistry in this country. It is true that the subject had not yet received due attention by the Universities and was being carried forward by the enthusiasm of individuals, such as Faraday and Graham, but when the British Association met in Newcastle 25 years later (1863), it was obvious that chemistry was a very lively subject and that the Section was full of activity.

Williamson was Sectional President and he was supported by Andrews, Lothian Bell (who was also Mayor of the City), Gassiot, Gladstone and Miller. Williamson summarised the ' business for which the Section meets—the consideration of papers and reports on special branches of chemistry and the chemical arts.' He drew attention to the enormous growth in the number of organic substances which had formed definite families —acids, bases, alcohols, ethers, etc.—' harmoniously united together by laws of composition.' He referred to Gerhardt's work and to Cannizzaro's, which had resolved the difficulties which, for so many years, had delayed a proper understanding. The molecule and the atom could now be defined. There were other exciting advances ; to mention a few —the discovery of the element thallium by Crookes, in which the new spectrographic methods of Bunsen and Kirchoff had played their part ; the Bessemer process for the manufacture of steel ; the discovery of aluminium (by Wöhler) and its use as aluminium bronze, and Perkin's new aniline dyes.

The immense progress which had been made in those 25 years was the subject of Sir William Armstrong's Presidential address to the Association. The progress in mechanical

engineering had had no parallel in history. The railways had come into being, the use of coal was increasing at the rate of 2¾ million tons per annum and had reached an output of 86 million tons. This, by the way, though less than half the total we now use, is very little less per head. Sir William was already expressing anxiety about the rate at which coal was being used. After slating the users of steam engines for producing so much smoke which was ' unnecessary and inexcusable,' he drew attention to the enormous waste of coal for domestic heating which, he stated, amounted ' to 1 ton per head per annum,' ' an abuse involving a useless expenditure exceeding in amount our income tax and capable of being rectified by attention to scientific principle.' I bewail with him. In the report of the Heating and Ventilating (Reconstruction) Committee, 1945, published by H.M. Stationery Office, it will be seen that there has been no improvement ; 1½ tons per head per annum was the figure for 1939 ; so although one may doubt the accuracy of Sir William's figure, the fuel burnt per head for domestic purposes certainly has not decreased. Sir William's address makes fascinating reading to-day ; so many things had happened in those 25 early Victorian—or Albertian—years ; the technological atmosphere had undergone a complete change. Nowhere had that been more apparent than in Newcastle. In science there had been huge happenings, the dynamical theory of heat—' probably the most important discovery of the present century,' as Sir William remarked—and the Darwinian theory had been launched.

Let us return to Section B for a moment, back to Williamson and the pain with which he referred to the disrespectful and unjustified remarks of the turbulent Liebig on Lawe's and Gilbert's experiments at Rothamsted, and to listen to some of the papers—Crookes on the recovery of a quarter of a hundredweight of thallium from flue dust, on the manufacture of aluminium by Deville's sodium process, Mr. Kessler on a new system of evaporating liquids, Dr. Condy on disinfectants and Dr. Riley on titanium in iron. An interesting meeting, that meeting in 1863 must have been.

William Armstrong, elevated to the peerage and about 80 years old, was present at the Association's meeting in 1889. Sir William Flower, a biologist and museum authority, was President. Section B was in charge of Lothian Bell who, now that science was unfolding so rapidly, took the advice of a former President of the Section, Tilden, not to attempt a catalogue of discovery but to elaborate a particular theme.

He naturally took the progress of the great national industry, the Iron and Steel Industry, as his theme. His address was one of profound interest, not only in his historical review of the development of the industry, but in demonstrating the dependence of that development on the application of chemical science. The law of mass action had been established and some of the technical puzzles about the behaviour of blast and steel furnaces came to be understood ; some, however, remain to be elucidated.

If we look for a moment at the business of Section B, we get an idea of the state of our science in England at that date. Dewar, the Franklands, Odling and Crookes were reporting on water analysis ; ultraviolet light, they found, produced some detectable hydrogen peroxide. Ramsay, Cundall, McLeod and Shenstone were investigating ozone. Dixon had a paper on explosions of hydrogen, chlorine and oxygen. Heycock and Neville were applying Raoult's law to the study of atomicity of molten metals. Richardson had devised a chlorine actinometer. Physical chemistry as a main branch of chemistry had been established; in fact it was in 1887, that Ostwald published in the first number of the *Zeitscrift für Physikalische Chemie* those two immensely important papers, the one by van't Hoff on ' Laws of Chemical Equilibria in the dilute or dissolved state ' and the other by Arrhenius ' On the dissociation of substances dissolved in water.' Nevertheless, one would not have quite gathered that these striking developments were happening from the records of the meeting of the Section. What does impress one, however, is the embryonic state of electro-technology. In the '60's there was nothing, but now they talk with bated breath of Crompton's 500 h.p. generator ' the largest machine in the world,' which was to be used by the Staffordshire branch of the Cowles' Company for the production of aluminium by a new electrolytic process.

We now come to the last occasion when Section B met in Newcastle, 1916, in the middle of the first world war. There had been another period of tremendous advance—an electrical era. The electron, X-rays, quantum theory, relativity, telecommunication, steam turbine and internal combustion engines—the discovery of the rare gases, radioactivity, great advances in organic synthesis . . . Great achievements and great powers thrust into man's hands, quite outside the grasp of those at the helm, and the world had gone to war.

Professor G. G. Henderson was our Sectional President. His address ranged wide. He spoke of the apathy with which governments had treated science and how it had needed the war to bring home the importance of the chemist. This led him to suggest ways

in which the newly devised Department of Scientific and Industrial Research might operate towards improvement. His recitation of the many advances in applied science brings us to modern times : the production of high and low temperatures, the electric furnace, the thermite process, the separation of liquefied gases, the oxygen industry, the fixation of nitrogen, the hydrogenation of oils and fats, the catalytic production of phthalic acid and acetaldehyde, the utilisation of formerly rare metals such as tungsten, and of rare substances such as nickel carbonyl.

The main business of the Section on this occasion was fuel economy. There was a strong committee presided over by my predecessor, Professor Bone, along with E. D. Simon (now Lord Simon of Wythenshaw), Sir George Beilby, Sir Richard Threlfall and Sir Robert Hadfield. They pointed out that Great Britain, once the foremost coal producer, now possessed only one-fortieth of the world's reserves, and they pleaded for a comprehensive survey of the coal seams and for the more efficient use of coal. Sub-committees were appointed, one under Bone for utilisation of the energy of coal, one under Merz for power and steam raising ; this committee bewailed, as I do to-day, the difficulty of obtaining data as to the amount of power, as distinct from heat, used in industry. One needs to know how much fuel is used for the three main demands, warmth, process and power. Simon had charge of another sub-committee on the domestic fuel problem, but no great improvements in this direction came about, though the committee instigated some experimental work, without which recent advances could not have been made. The Section had a joint discussion with the Geological Section on the chemical and geological characteristics of different varieties of coal, to which Stopes and others contributed. These discussions were useful, for after the war the Coal Survey came into being.

The British Association has ever urged the study of the conservation and efficient utilisation of our resources, such as fuels, or water, or soil. In times past, it was like shouting into space—no echo came back : nowadays, there is machinery, both national and international, for reception, study and, one hopes, right action on such questions. In a world which is undergoing technological changes so rapidly, such machinery is essential. Only by close and continuous study of the repercussions of the new upon what exists can good use be made of the harvest of invention and the progress of discovery. At the present time, the advent of the gas turbine, the possibilities of improved methods of total gasification of coal, the rapid expansion of electrical power generation, the processes for production of oil from coal, the transformation of oil and fuel into chemical products, etc., provide problems which require close and continuous consideration of this kind.

Thirty-three more years have now gone by since that meeting of the Association in 1916 ; a second war has scarred the world. The Association and Section B are again welcomed in Newcastle. Williamson, in 1863, had spoken of the atom as ' groups which we cannot break up, or any reason why we should ever break up.' Now the atom is split —we have been projected into the atomic, or nuclear, age. Our destiny is in our hands. More power than ever is thrust upon man.

I shall not attempt to catalogue the wonderful advance in chemical science during this recent great era of scientific achievement, it is all so immense, ornate and close. Before embarking, however, on the main theme of my address, I could not resist the temptation to peep through the crannies of the castle wall at the passing of the years and to paint, as it were, the scenery against which my talk ' on the influence of combustion on the progress of chemistry ' might take on more life.

Fire has ever been man's spur towards adventure and experiment. Of the four elements of the ancients, fire had first place ; ' Remember ' wrote Marcus Aurelius, ' the saying of Heraclitus, that the death of earth is to become water, the death of water to become air, and the death of air is to become fire.' It was only natural that the successive stages of civilisation—the bronze age and the iron age—were related to the ease of reducibility of ores by fire, and that the chemical arts in the hands of the alchemists were associated with the furnace and the bellows. Coming to a time of greater enlightenment, it is somewhat surprising, considering how close to the truth were Boyle and Hooke's views about flame and combustion towards the end of the seventeenth century, that it took yet another century before the specious phlogiston theory—an inverted conception—was eventually overthrown by Lavoisier and chemistry became a science.

It is not easy to get a correct historical perspective on the state of contemporary knowledge in a subject. The knowledge available to a person at any given time is not a measure of the state of advance at the time, for he may be ignorant of much that has been done ; on the other hand, in the particular field of his own enquiries, he may be far ahead. The history of science written from the standpoint of the achievements of individuals and integrated therefrom, may not, therefore, be in quite true perspective. For instance, in Lavoisier's *Elements de Chemie* (1789) there is no mention of Cavendish's

previous work on the constitution of water, while Cavendish himself accepted the phlogiston theory which Lavoisier overthrew on the basis of the kind of evidence which Cavendish had recorded. Nicholson's *Dictionary of Chemistry* (1795), which was a book Davy used, makes no mention of Black's researches, published forty years before, which we look on nowadays as one of the outstanding early advances.

'Priestley, Cavendish, Lavoisier and la Révolution Chimique' was the title of Sir Edward Thorpe's address to the Section in 1890 following the Newcastle meeting. Although of prime importance to my theme, it is quite unnecessary to repeat the story of the overthrow of the phlogiston theory, and the birth of chemistry which resulted from the understanding of the rôle of oxygen in combustion.

We will start to develop our subject with Davy's work. We may remember that a dinner had been held in honour of Sir Humphrey in Newcastle on the 13th September, 1817, two years after the invention of the safety lamp. Sir Humphrey, in his speech, said that 'science in the hands of the chemist and mechanic has made pit coal—the elements of fire and water—perform operations formerly demanding human labour, and has converted the productions of the earth into a thousand new forms of use and beauty . . . allow me to observe that it was in pursuing the methods of analogy and experiment that I was fortunately led to the invention of the safety lamp.' Referring to the publication of his papers on the subject in the Transactions of the Royal Society, he ended by proposing the 'Prosperity of the Coal Trade.' Is it not still possible to catch the enthusiasm of his sentiments and instil into all those who have to do with coal in our days the thrill of their labours, both in the getting of the coal and in its utilisation? The safety lamp might have been devised by others—in fact, George Stephenson invented such a lamp about the same time—but the researches on combustion associated with Davy's invention could only have been done by a master hand.

Davy's Researches on Flame were published in the *Transactions of the Royal Society* in January 1817; they are the foundation on which the study of combustion as a special branch of chemical science has since developed. If one gauges the state of chemical knowledge about this time, one realises the mastery they show. Davy was only 37 at the time. One must remember too that heat was still very much a mystery. Davy examined the limits of combustion, determined the velocity of flames, the ignitability of mixtures, studied the effect of inhibitors of combustion, attempted the determination of the temperature of flames by the rise of pressure during explosion, studied catalytic combustion at surfaces, investigated the production of carbon in flames and made some observations on the phenomena of slow combustion. Did this work affect the progress of chemistry? It did in this way: chemists knew, after this work of Davy's, what they were about in dealing with flame and explosions, and it helped in the understanding of the nature of heat which came about during the subsequent thirty years, culminating about 1850 in that great achievement of the century, 'the statement of the laws of thermodynamics.' The main advance in chemical science during this period was towards expansion of chemical species in the mineral and the organic realm, the systematisation in terms of the atomic theory and the development of methods of analysis, but there was other work which was sowing the seeds of future achievements, for instance, Faraday's researches on electrolysis. Catalytic combustion was also in this category—a subject on which Davy, among others, did notable work. It did not come into much prominence again till the end of the century, Sabatier's researches having opened the way to great modern developments.

Gas analysis was a technique necessary to the progress of systematic chemistry; it involved combustion and measurement of changes of volume. After Gay Lussac's early researches, it was Bunsen to whom we mainly owe the advance made. Bunsen and Playfair's investigations on blast furnace gas, reported to the British Association in 1845, were an outstanding contribution to scientific metallurgy. The aerated gas flame would have come, whether or not Bunsen had been urged to devise it for installation in his new laboratory at Heidelberg. Faraday had already used something very similar. Nevertheless, we owe its development to Bunsen, and ever since 1855 it has been a necessary tool in the chemical laboratory. This aerated gas flame had its influence in the development of spectrochemistry and led to the discovery of thallium by Crookes, rubidium and caesium by Bunsen. There was another striking invention, that of the matches which light the gas; these had their origin in John Walker's friction lights which were first sold in 1827 at his shop in Stockton-on-Tees.

Bunsen and his school made substantial contributions to the study of combustion, but they were just further developments along the lines of Davy's work in tune with the advance of chemistry as a whole. His work enlivened enquiries which eventually helped to establish the law of mass action, just as his attempts to measure specific heats of gases by explosion methods were on a road which led

to important developments in chemical thermodynamics. His attempts to measure the velocity of flames by stabilisation of a flame front were indeed valiant, but not very successful.

We come now to the period of the 1889 meeting of the British Association—the period in which the law of mass action had been established and the conception of chemical equilibria in accordance with thermodynamics had been developed, particularly by van't Hoff. A big advance in combustion chemistry had been made. This time in France. Berthelot had discovered the detonation wave (1881) and Mallard and le Chatelier (1883) had published a comprehensive memoir in which they described photographic methods of studying flame propagation, amongst other important innovations. There followed on this a fresh interest in combustion which has never waned. This work of the Frenchmen was the third big step in combustion chemistry. Can we indeed yet say exactly how a detonating type of combustion becomes established and by what precise mechanism flame is propagated?

Next in the period 1889-1916 comes H. B. Dixon's work at Oxford and subsequently at Owen's College, Manchester, on the combustion of carbon monoxide and the significance of water vapour; his repetition of Dalton's study of the combustion of ethylene, which disproved views which had long been held about the preferential combustion of hydrogen, and his extensive studies of the detonative type of combustion. About this same time, Smithells, Teclu and others were investigating the process of combustion in the bunsen flame; they succeeded in separating the outer and the inner cones and in making analyses of the interconal gases. This work helped to improve appliances for the combustion of gas and to provide further information about chemical reactions in flames. More important was the knowledge, provided by all these studies of combustion along with thermodynamical measurements which enabled physical chemists, such as Le Chatelier, Haber and Nernst, or engineers such as B. Hopkinson to advance the understanding of the working of furnaces and engines. The temperature and dissociation of the exploded gases in an engine, for instance, could be determined and the theoretical power and efficiency could be compared with the measured quantities. Victor Meyer and later Dixon had attempted to improve the methods of finding the temperature at which a combustible would ignite in oxygen or air, but it was at Nernst's suggestion that the adiabatic method was first applied: by using the heat generated in the sudden compression of a gas, its temperature of ignition could be determined without being influenced by surface effects. The method was improved later by Tizard and Pye.

Nernst and others used explosions in closed vessels to determine specific heats of gases at high temperatures, and this work had results of importance to chemical theory: the estimation of molecular moments of inertia of the molecule and the calculation of equilibria.

In flame reactions, everything takes place so rapidly that it is difficult to unravel the process by which the final flame products are formed. W. A. Bone was one of the first to attempt a systematic study of the products of explosive combustion of hydrocarbons and later to study their slow flameless oxidation. He was the first to recognise the importance of analysis of the various intermediate products formed, which could provide a clue to the path of the reaction. As a result of much painstaking work, he was led to the view that the process of oxidation was one in which the hydrocarbon became successively more hydroxylated: a view which has since been modified.

In technology, the development of the gas industry, coke ovens and producers, improvements in boiler and other furnaces, the use of acetylene, the rapid expansion of the petroleum industry, the development of internal combustion engines and the increased use of solvents had provided much incentive towards combustion research.

This brief review brings us to 1916, when the British Association last met in Newcastle, and I hope enough has been said to indicate the impact of the study of combustion on the general progress of chemistry up to this time. British chemists, following Sir Humphrey Davy, had indeed maintained a lively interest in the subject.

If it is agreed that during the last century the study of combustion along the lines initiated by Davy had a considerable repercussion on the progress of chemistry, it will certainly be easier to establish its influence during this most recent period of thirty years. The laws of chemical equilibria and the determination of molecular structure had been the main achievements; chemical dynamics was still a territory which was largely undeveloped. When gases combine, when a gas vaporises or condenses, when a crystal forms, or a solid melts, when a substance dissolves, when a metal yields, what exactly occurs? The work of van't Hoff (1884) and Arrhenius (1889) had prepared the way to an understanding of the dynamics of gas reactions, but van't Hoff had found that the majority of chemical reactions do not follow the simple transformation laws which he had formulated. He was led to study what he

called perturbations from the normal behaviour. Arrhenius' great contribution was to point out that not all molecules react, only those with an energy greater than a certain value (E). The reaction depended on this activation energy. The exciting time, however, was between 1916 and 1923 ; it was not clear how a unimolecular gas reaction, in which the rate of reaction is independent of the pressure, could occur. The decomposition of nitrogen pentoxide was a case in point. The radiation theory discussed by W. McC. Lewis (1916) and Jean Perrin (1919) succumbed to criticism and was replaced by the theory of activation by collision suggested by F. A. Lindemann and brilliantly supported by the experimental work of C. N. Hinshelwood. When the average time between activation and chemical transformation is large compared with the time between one collision and the next, many of the activated molecules lose their energy by collision before they have had time to change, so the reaction is independent of pressure until so low a pressure is reached that the number of collisions becomes insufficient. The other main advance took longer to impose its significance. Bodenstein (1913) was the first to mention the notion of a ' reaction chain,' and Nernst (1918) used it in discussing the hydrogen and chlorine reaction, which was found to give a large yield in relation to the light absorbed ; chlorine atoms reacting on hydrogen generate hydrogen atoms to react with chlorine, thus continuing to produce hydrochloric acid and to release an atom at each step to carry on the reaction. Christiansen and Kramers, working in Niels Bohr's laboratory, in 1923, applied the chain idea to provide an alternative explanation of unimolecular reactions. The explanation landed into difficulty when applied to the unimolecular decomposition of nitrogen pentoxide, but its importance revived when the Russian physicist, Semenoff, in 1928, applied it to explain the behaviour of phosphorus on oxidation. He used ' the branching of chains ' to explain the sudden onset of explosive combustion at a certain limiting pressure. When more than one radical, e.g. OH or H, is produced in a reaction and each can continue to propagate a chain reaction and so lead to further branching, then the reaction accelerates so quickly that an explosion results. This notion was soon applied to explain the explosion pressure limits which Hinshelwood and H. W. Thompson had discovered in studying the combustion of hydrogen and oxygen at low pressures. It was also obviously applicable to the results which S. F. Gates and I had got in studying the ' knock ' and ignition of hydrocarbons. From then to this day, the phenomena of combustion (slow combustion and ignition) have provided an almost inexhaustible field in which these kinetic theories of reaction can be applied. ' Almost inexhaustible,' because there are very large numbers of hydrocarbons and combustible substances and each can give rise to numbers of intermediate oxidation products, many of which not only themselves undergo combustion, but also influence the reactions which have produced them.

It had been learnt from the work of Dixon and of Bone, that oxidation was subject to delay, to a so-called period of induction preceding combustion. Semenoff supplied an explanation for this : intermediate substances formed as a result of the main reaction, have to build up to a certain concentration before their own decomposition becomes sufficiently rapid to provide reacting centres from which the main reaction hastens out. The build-up may take quite a long time. The process was called a ' degenerate explosion.'

The study of slow oxidation and ignition has in recent years provided a most prolific field of enquiry, which has enriched chemistry by the kinetic approach in distinction to the thermodynamical approach of the latter part of the last century. Such studies have had their repercussion in other directions ; for instance, in the chemistry of polymers, the building of large molecules from small units, which has been much to the fore recently not only because of its technological importance in the plastics, paint, rubber, textile and other industries, but for its own scientific interest. Polymerisation and depolymerisation are influenced by oxidation : the oxidation may not only influence the rate, but may change its course.

Oxidations in the liquid state are subject to similar chain reactions as occur in the gaseous state. It seems to be becoming generally agreed that in all these oxidations, the initial steps are dependent on peroxidation. The reacting molecule is first shaken by collision or otherwise, so that a hydrogen atom becomes detached, oxygen steps in and forms a peroxide radical which on encounter with another molecule, gives a peroxide, or if this is not stable enough to exist, the resulting complex breaks up into simpler components. Even the oxidation at low temperatures of a solid like carbon is preceded by formation of peroxidic complexes on the surface. In fact, there seems to be some underlying theme of this kind common to combustible substances from which nature constructs a symphony of behaviour of great variety. In the oxidation of quite a simple hydrocarbon such as n.octane, the stages of breakdown may involve the formation of a number of different peroxides, but it is not so much their formation but the manner of their break-up

that determines the main course of the oxidation. The influence of inhibitors and promoters on the oxidation, which has been of importance in the study of ' knock,' is linked with the formation and stability of these peroxides. There should be connection here with biological processes of oxidation and polymerisation which occur at normal temperatures in the presence of catalytic agents, such as glutathione or co-ordinated metal groupings, and one feels that in this field there is much yet to discover.

Nowadays chemists do not accept the mere existence of atoms, radicals and molecules ; they have to consider them as electrodynamic systems subject to quantum dynamical laws, described in terms of atomic and molecular orbitals, and subject to patterns of behaviour determined by shift of electron density. The study of reaction kinetics, which includes combustion and oxidation, has had a great influence towards this closer understanding of the true nature of chemical change, and this in turn has had its effect in advancing knowledge of molecular structure and reactivity.

It has not been my intention to review the great advances in Chemistry made in the last twenty years, but I have intended to point out the connection it has all had with the study of combustion. The effect can be seen in other directions. I have already mentioned that important work was done on the chain theory of reactions in Bohr's laboratory in 1923 ; it was to the fore there again when the possibility of propagating nuclear fission by chain reactions of fission products came to be considered. The consequences of this to the world have been terrific. At all events, chemistry has been enriched by the filling of all the gaps in the periodic classification of the elements (43, 61, 85, 87), the provision of a new set of transition elements of atomic number greater than uranium (93 to 96) and large numbers of isotopic elements, stable and unstable, which provide further means to trace where and how reactions occur.

The combustion chemist is usually concerned with rapid reactions because of the many practical problems involving detonations, explosions or flames. It is not easy to trace what happens in these very fast reactions. In slow combustion, from pressure change and determination of the substances formed, it is possible to understand what is happening and even to predict when, by branching or self heating, the mixture will trigger over into an explosion and ignite.

A very complete study of the hydrogen and oxygen reaction has been made in this way by Hinshelwood and his school. But to determine just what happens in an explosion or in any very rapid reaction associated with flame is not an easy experimental problem. Polanyi, however, showed how it was possible to study fast reactions in flames at low pressures. His were sodium vapour flames burning in halogen or alkyl halides, and from the limit of the ambit of the sodium as found by its fluorescence, it was possible to determine the course of a reaction and the effectiveness of the collisions for reaction between the atoms and molecules. Gaydon and Wolfhard in my Department at South Kensington have been able to maintain stable hydrocarbon flames at quite low pressure. The zone of the flame reaction can be spread into quite a wide band, as the free path of the molecules is appreciable. The position where the various radicals, which emit light, commence to show themselves and their temperature can be found. Although one cannot get the whole story in this way, much about these fast reactions can be discovered. It is possible also to freeze out suddenly and analyse the products formed in a flame and so get further information about the processes ; this is another line which Minkoff and Everett have been investigating in our laboratory. Others —Coward and workers in the Safety in Mines Laboratory, Laffitte and others in France, Stevens, B. Lewis and G. von Elbe and others in America, Linnett, Boys and Corner, recently in England, have advanced our knowledge of flame propagation.

I have mentioned these different ways in which something is being found out about flame reactions, but we are far from an exact knowledge of what Robert Hooke called ' that shining transient body,' flame. In ordinary flames, it seems that the propagation is maintained by the diffusion back into the unburnt gas of some of the radicals from the flame, rather than by the self-acceleration of the ignition reactions in the unburnt gas alone. There may thus be a distinction between inflammation, which determines flame propagation, and ignition. Nevertheless, when the gases can be self-heated quickly at low temperature, inflammation may become a double process (involving self-ignition and subsequent inflammation), as Townend has shown with ether and acetaldehyde.

We are having a discussion about flames later and I only refer to this aspect of combustion chemistry as the subject of my address would be incomplete without some mention of it, and this ends my review of the influence which the study of combustion has had on the progress of chemistry ; all along great technical developments have closely followed this growth of knowledge.

Let us return once more to Davy and learn something from the way in which he tackled his problem. May I remind you of the story : Davy was on a visit to Melrose, where he

was shooting with Lord Somerville. A letter came to him there from the Reverend Dr. Gray, who was Rector of Bishop-Wearmouth, asking him to help in finding some way of combatting the dangers of firedamp in the mines. There had been a terrible accident three years before at the Felling Colliery which had struck horror.

Davy offered to call at Newcastle and visit the mines on his way south ; Mr. Buddle, a noted mine manager, met him on the 24th August and took him down the mine at Wall's End. Davy asked for samples of the firedamp and as soon as he reached London, he set to work. Within a fortnight he had determined the limiting compositions for combustion of the gas and had shown that it would not explode in a narrow tube. He wrote to Dr. Gray in October to say that he had discovered unexpected properties in the gas which had led him to no less than four different plans for lighting the mines with safety. He went on with his researches and was able to send lamps for trial early in January. Excusing his procrastination, he wrote ' it is impossible to conceive the difficulty of getting anything made in London which is not in the common routine of business.'

His first paper on the subject was read to the Royal Society on the 9th November, 1815, and in a second paper on the 11th January, 1816, the gauze protected flame was described. In February 1817, his great paper on ' Some new experiments and observations in the combustion of gaseous mixtures ' was read, in which he summarised his researches and which, as I said in my earlier remarks, formed the starting point for all subsequent work on combustion. The whole is a perfect example of a piece of technical research ; the fixing of the practical problem, the preliminary experiments to feel about for the right line on which to tackle it, the solution and then the follow up to find out the underlying principles. Since those far-off days there have been many, many similar examples of science advanced in just the same way. An outstanding example about a hundred years later was Langmuir's tungsten filament lamp, leading to his remarkable work on heterogeneous catalysis.

But there is the other way in which technological advance comes about. I am thinking, for instance, of the improvement of the aero-engine : how gradually, taking advantage of progress in science—in thermodynamics, in metallurgy, in elasticity and mathematical theory of the vibration of structures and in numerous other directions —the engine eventually surpassed undreamt standards of achievement. The same could be said, I suppose, of a process like the ammonia soda process or the synthesis of ammonia.

The impetus which technology gives to science and the impact of science on technology form the dual process which has produced in these 150 years the stupendous powers over the forces of Nature which man has achieved. But the pace quickens ; it is well to remember that chain reactions can lead to explosion. If our highly developed state of civilisation is to remain and to extend to other less civilised regions, much needs to be done to conserve the world's resources and to avoid the wanton and wasteful plunder of the past. Conservation is not just a question of postponing future difficulties, but there is need for it for the other economic reason—the present efficient use of man-power. There is no sense in wasting that which needs labour to win. There is much to be done, therefore, in the conserving and effective utilisation of our own resources of fuel Now that the fuel industries belong to the nation, it should not be impossible to work in harmony with such aims. Effective utilisation means satisfactory load factors, economical distribution and appropriate allocation of fuels in relation to quality, purpose and locality. As technology is never stagnant and new possibilities continually present themselves, continuous study and research are necessary, so that wise advantage can be got from new developments.

It is not within the theme of this address to discuss these questions, but an illustration may be given of the kind of intricate problem which calls for consideration. What prospects would there be for the gasification of coal at the pit head by a process of total gasification and for its distribution at a high pressure to a distance ? There are several different processes of total gasification, but none is so far developed that an unequivocal answer can be given to the question. But the posing of the question is sufficient to indicate the important possibilities that arise. Underground gasification, the production of oil from coal, the storage of energy in the form of liquified methane, use of pumped storage and of tidal and wind power are amongst other such questions.

I have referred earlier in this address to the statement that about a third of Britain's coal production has been used for domestic heating. The inefficient use of coal for this purpose is a drain upon our economy, and leads to needless dirt and gloom. There is still much to do to improve the efficiency and I shall not be happy until the amount of coal used for this purpose is reduced to a more reasonable total without restriction of necessary standards of warmth and amenities. The problem is intricate, but recently some fine scientific work has been done on it at the

Government Research Stations which should make improvements easier to achieve.

Although our fuels must, in this highly-populated state of our country, be our mainstay, it should be realised that fuel and food are all part and parcel of a single economic story. Both fuels and food provide warmth, which is essential to life, and they also provide power, the power of the machine which supplements labour and the power of muscle.

It is necessary to remember too that our fuel is a capital legacy from the sun, which provides us with an income also. It is not outside the possibilities of science to find means to increase the income that we receive, to increase the photo-stimulus of solar radiation. There is much yet to learn about the wonderful processes by which the plant builds up at ordinary temperatures so many products, for which we seem to use cruder and more wasteful processes. It may be that it is in these directions that the future may be rich in prospects. Looking back to the turn of the century and remembering the state of chemical science at that time, then seeing in my mind's eye the integrated achievements of chemists since those days, I cannot but believe in a bright future :—

> ' That which they have done but earnest of the things that they shall do.'

SECTION C—GEOLOGY

RECENT WORK ON THE LOWER PALAEOZOIC ROCKS

ADDRESS BY
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PRESIDENT OF THE SECTION

THE principles of stratigraphy were established in this country about 150 years ago by William Smith who, in 1815, published his geological map of England and Wales. At this time, the general succession of the stratified formations down to the Carboniferous and the Old Red Sandstone had been determined but the chronological order of the older rocks was unknown. In 1831, Roderick Murchison in the Welsh Borderland and Adam Sedgwick in North Wales began to investigate these older rocks, which they assigned to the Silurian and Cambrian Systems. Then followed the controversy concerning the relationship between these two systems and, in 1879, the third great pioneer of Lower Palaeozoic stratigraphy, Charles Lapworth, proposed the Ordovician System for the upper part of Sedgwick's Cambrian and the lower part of Murchison's Silurian ; so arose 70 years ago the familiar three-fold division of the Lower Palaeozoic rocks now adopted in most parts of the world.

Many workers have been attracted to the problems presented by these rocks and we have a general picture of the conditions which prevailed in Great Britain during their formation. The Lower Palaeozoic was a period of marine sedimentation, accompanied by extrusive and intrusive igneous activity which, according to some estimates, began about 500 million years ago and endured for nearly 200 million years. It was preceded by the Pre-Cambrian, which was some three times as long as the rest of geological history, but the Lower Palaeozoic rocks are the oldest in which fossils can be used for classification and as evidence of conditions of deposition.

The history of the Lower Palaeozoic is the evolution of a geosyncline with its marginal shelf-seas which extended from south-west to north-east across the western part of Great Britain and in which accumulated a great thickness of sedimentary and volcanic rocks. This prolonged cycle of sedimentation was closed at the end of the Silurian by the compression of the geosyncline and the deposits laid down in it were elevated to form the Caledonian folded mountain ranges. These ancient mountains were destroyed by erosion in later periods but the structures seen in the Lower Palaeozoic areas were, in the main, produced by the Caledonian earth-movements, structures often complex which have to be unravelled by the stratigrapher.

We shall deal with some aspects of the work which has been carried out on these rocks in Great Britain during the past ten years or so but, in addition, we shall make use of the results of earlier investigations in order to give a general account of the physiographical conditions.

CAMBRIAN

The Cambrian rocks are found in Wales, the Midland Counties of England and Scotland ; there are certain other areas where rocks have been assigned, without definite evidence, to this period.

Wales

The rocks crop out in North Wales in the Harlech Dome and reappear in Caernarvonshire, whilst in South Wales they are seen in Pembrokeshire, especially at St. David's.

Charles Lapworth worked for many years on the Harlech Dome ; C. A. Matley and T. Stacey Wilson (1946) have completed and made available Lapworth's mapping. The base of the system is not seen but the succession described by them extends to nearly the summit of the Lingula Flags, while W. G. Fearnsides and W. Davies (1943) have dealt with the Upper Cambrian on the north-western side of the dome. These papers give detailed information concerning the lithological and faunal sub-divisions ; the Lower Cambrian *Olenellus* fauna, however, has not been found. The maximum thickness of rocks described by Matley and Wilson is more than 10,000 feet and it seems likely that the total thickness of the Cambrian is about 15,000 feet, the thickest development which is known in Great Britain. Their geological map affords valuable information concerning the structure of the dome and the distribution of the numerous intrusions but much further work remains to be done on the

petrography and intrusive habit of the igneous rocks.

A. W. Woodland (Appendix in Matley and Wilson 1946) has described the petrology of the sediments in the Harlech Dome. He has shown that the manganese ore-bed, a banded stratum about a foot thick in the Manganese Shales of the Harlech Series, is of sedimentary origin with some later chemical reconstitution. The red and yellow layers consist essentially of spessartite-garnet and dialogite in about equal proportions with, in rare instances, subordinate rhodonite ; the colour of the red layers is due to small quantities of haematite which is practically absent in the yellow layers, while the black bands contain some pyrolusite.

The mudstones and grits associated with the ore-bed are also garnetiferous and garnet is found at higher levels in the Harlech Series and in the Menevian. Woodland considers that the general re-crystallisation of the argillaceous materials in the Harlech Series as chlorite and sericite indicates metamorphism equivalent to the chlorite zone of regional metamorphism. At that stage, spessartite-garnet may be developed if a considerable amount of manganese is present ; in the ore-bed, the dialogite is regarded as representing the excess of manganese remaining after the aluminous materials had been used up in the formation of garnet. He would attribute the metamorphism to the pressure these rocks were subjected to owing to the great thickness of later Lower Palaeozoic sediments deposited upon them ; but temperature is likely to have been as effective as pressure.

Woodland concludes that the land-mass from which the Cambrian rocks were derived was in general similar to the Pre-Cambrian complex of Anglesey but that it probably included more extensive exposures of granites and gneisses as well as of volcanic rocks of trachytic and keratophyric types, whilst metamorphic rocks now seen in the Penmynydd zone were correspondingly less important. He considers that the general field-relations of the Harlech Series, especially the distribution and easterly thickening of the grits, indicate that the bulk of the sediments came from an easterly direction.

O. T. Jones (1938) points to the great reduction in thickness and in grain-size of the Cambrian rocks in passing from North Wales to the Midland Counties of England and suggests that the pebbles of Anglesey type in the Harlech Series may have been derived from that island or a continuation of similar rocks in some part of what is now the Irish Sea. We shall mention later that much of the Irish Sea and adjacent regions were probably land in Cambrian times and the Pre-Cambrian rocks of Anglesey may be an incomplete picture of the rock-types which occupied it.

The Cambrian rocks of Caernarvonshire, which rest unconformably upon Pre-Cambrian, begin with about 2,500 feet of conglomerates and grits, followed by a slightly greater thickness of purple and green slates ; they are succeeded by the Bronllwyd Grits, up to 1,500 feet thick, and these are overlain by about 2,000 feet of sandy sediments representing a part of the Lingula Flags, the upper part of the latter and the whole of the Tremadoc being absent ; the maximum thickness is over 8,000 feet but the succession is incomplete. The highest slates below the Bronllwyd Grits have yielded '*Conocoryphe*' *viola*, a trilobite formerly taken to indicate Middle Cambrian but now regarded as of Lower Cambrian age ; some Lingula Flag fossils have been recorded from well above the grits but the characteristic Middle Cambrian fauna recognised in the Harlech Dome is absent. The Bronllwyd Grits have been correlated with the lower part of the Lingula Flags and the rocks below them with the Harlech Series and the Menevian of the dome, but O. T. Jones (1938) regards the grits as probably equivalent to the Rhinog Grits carrying the implication that much of the succession in Caernarvonshire is older than that exposed in the Harlech Dome. He would regard the failure of certain subdivisions in Caernarvonshire as possibly due to pene-contemporaneous erosion on approaching the land-mass from which the sediments were derived ; a satisfactory correlation would give valuable evidence of the palaeogeography.

In South Wales, the detailed succession from the base of the system into the Lingula Flags has long been known ; the proportion of grits and the thickness of the Lower and Middle Cambrian are much less than in the Harlech Dome, suggesting a facies intermediate between that area and Shropshire. O. T. Jones (1940) has shown that there is almost certainly an unconformity between the Middle (Solva) and the Lower Cambrian (Caerfai) at St. David's and this discordance appears to be present elsewhere in Pembrokeshire.

Midland Counties of England

The Cambrian rocks appear in relatively small outcrops of which the most important are Shropshire, the Malverns and Nuneaton. They are not known north of a line from Shrewsbury to Leicester ; they occur beneath the surface near to and south of Leicester but not apparently farther north. Tremadoc rocks have been proved as far south as the Tortworth inlier in Gloucestershire and are

also known in the Calvert boring in Buckinghamshire.

We need only mention one or two salient features. The lithological succession, which falls into a lower arenaceous and an upper argillaceous part, is distinct from that in Wales but the faunas are closely comparable in both regions. The thickness is much less than in North Wales; it may have been originally between 6,000 and 7,000 feet, the smaller exposed thickness of about 4,000 feet at Nuneaton being due to the unconformity which conceals the highest rocks and in Shropshire because of interruptions in the succession. The thickness variations in the Tremadoc, however, are independent of those in the underlying rocks so that 800 or 900 feet in the Harlech Dome are represented by more than 3,000 feet of Shineton Shales in Shropshire.

Scotland

Lower Palaeozoic rocks of an entirely different facies from those in Wales and England are seen in the North-West Highlands, where they form a narrow outcrop about 100 miles long, rest unconformably upon Torridonian and Lewisian and are terminated eastwards by the Moine Thrust. The succession begins with an arenaceous series, up to 700 feet thick, which is Lower Cambrian, but the faunal affinities are with North America rather than with the Welsh-Midlands region. It is succeeded by the remarkable Durness Limestone, about 2,000 feet thick and divided into seven groups. *Salterella* has been found in the lowest group and since that fossil occurs in the arenaceous series the limestones yielding it have been regarded as Lower Cambrian; it is possible that *Salterella* may also occur in the higher second group. The fossils in the remainder of the Durness Limestone consist mainly of gastropods and cephalopods and are most abundant in the fifth and sixth groups; they indicate, by comparison with eastern North America, that these rocks must be regarded as younger than Cambrian, being equivalent to the Arenig and possibly higher stratigraphical levels.

The Highlands east and south of the Moine Thrust are occupied by the Moinian and Dalradian metamorphic rocks. The Moinian is accepted by British geologists as Pre-Cambrian and the Dalradian, which makes a broad belt across the Southern Highlands, has been regarded as also of Pre-Cambrian age. J. G. C. Anderson (1947) has recently described the geology of the Highland Border; on the north side of the Highland Boundary Fault, altered sediments belonging to the uppermost Dalradian can be divided into an older Pelitic and a younger Psammitic Group. The Pelitic Group consists of slates, phyllites and mica-schists with, in places, psammitic bands and thin limestones; it includes the Aberfoyle Slates in Perthshire. This group passes upwards through an interbedded junction into the Psammitic Group which is divided into Lower Grits. Leny Limestone and Shales and Upper Grits; the grits show graded bedding which can be used to determine the order of succession.

J. Pringle (1939) found Middle Cambrian trilobites in the Leny Limestone of the type-locality near Callander, including *Pagetia* and other genera, but a description of the fauna is not yet available. It is suggested by Anderson that both the Pelitic and Psammitic Groups are of Cambrian age; it is still uncertain, however, whether Dalradian rocks older than the Pelitic Group are also of that age. He points out that the frequent occurrence of coarse, even conglomeratic, grits in the Pelitic and Psammitic Groups show that the land-mass from which they were derived was not far away; such evidence as is at present available suggests that the sediments were derived from the south and from an area which consisted of coarse granites or orthogneisses of an acid type. Pringle (Discussion in Matley and Wilson, 1946) claimed that there is a strong resemblance between these Dalradian rocks and the Lower and Middle Cambrian of the Harlech Dome but *Pagetia* is an American form which is not known elsewhere in Great Britain, suggesting that the Highland Border seas were separated from those in Wales.

North of England and South of Scotland

There is no clear evidence that Cambrian rocks occur in England north of the Welsh-Midlands tract. It has been claimed that the lowest Skiddaw Slates may be Upper Cambrian (Tremadoc) because of the occurrence of the dendroid graptolite *Bryograptus* but the main mass of these sediments is clearly Ordovician and it seems unlikely that any part of them can be assigned to the Cambrian. The Manx Slates, which occupy the greater part of the Isle of Man, were at one time thought to be the equivalent of the lower part of the Skiddaw Slates, but no diagnostic fossils have been found in them and it seems preferable in the present state of our knowledge to refer them to the Pre-Cambrian.

The Southern Uplands consist of Ordovician and Silurian rocks but the base of the Ordovician is nowhere seen. There are a number of intrusions in the Lower Ordovician (Arenig) of the Girvan-Ballantrae area which are usually regarded as of Ordovician age but J. Pringle (1948) has suggested that some of them may in fact represent part of

the Pre-Cambrian floor upon which the Ordovician sediments were deposited ; elsewhere in the Midland Valley there are only inliers of Silurian rocks.

General

It has been claimed that the widespread, shallow seas of early Cambrian times were followed by the development of deeper water conditions over the Irish Sea–Lake District region and that the latter constituted a barrier between the contrasted shallow water faunas of the Welsh-Midlands tract and Scotland. Certain considerations suggest, however, that a land-mass, probably of considerable size, separated the Cambrian seas of the Welsh-Midlands tract and Scotland.

In the Welsh-Midlands region, sedimentation was greatest in the Harlech Dome, where the sea-floor subsided some three miles during the Cambrian period and conditions were geosynclinal. Shelf-seas spread east and south, affording free communication for the faunas. It seems likely that the main mass of the sediments was derived from the north and north-west.

Away to the north lay a separate basin in which the North-West Highlands rocks were deposited but only the lower part of the Durness Limestone appears to be possibly of Cambrian age. The discovery of Middle Cambrian fossils in the uppermost part of the Dalradian means that a portion at any rate of that system is Cambrian. The sediments are a greywacké-shale facies of geosynclinal type which appear to have been derived from the south.

It seems reasonable to infer that a considerable tract of land separated the Scottish area of deposition from that of Wales and England. It probably covered most of the Irish Sea and may have extended westwards into Ireland and eastwards under the Pennines ; we may be catching a glimpse of it in the Ingletonian, while F. W. Cope (1948) has shown that the Carboniferous Limestone of the Derbyshire Dome rests at depth upon what is almost certainly a Pre-Cambrian floor. It is not impossible that it may have extended into the Southern Uplands, since the Cambrian rocks of the Highland Border were derived from land which was probably not far away to the south of them.

This land-mass was doubtless the main source of supply of the Cambrian sediments which were deposited to the south and north of it ; to the south the thick development of North Wales and to the north there may have been an equally thick development of a comparable greywacké-shale facies. Maximum sedimentation may thus have taken place on each side of and adjacent to this Pre-Cambrian land-mass, truly geosynclinal conditions, whilst to the south and to the north a smaller thickness of deposits accumulated in shelf-seas, but it is not known whether the Highland Border seas were continuous across central Scotland with those in the North-West Highlands, although there is likely to have been communication between them. Movements of this Irish Sea land controlled the characters of the sediments deposited on each side of it, coarser grained when it stood high and finer grained when it was of low relief ; it was an area of geanticlinal character.

Ordovician

The Ordovician rocks are found mainly in Wales and the Welsh Borders, the Lake District and Scotland ; there are also important outcrops in Ireland. There are two facies, a shelly, usually arenaceous and sometimes calcareous deposited in shallow water, and a graptolitic, usually argillaceous deposited farther out to sea ; considerable thicknesses of contemporaneous volcanic rocks may be associated with them.

Wales and the Welsh Borders

The rocks occupy most of south-west Wales and extend into the eastern part of Central Wales along the Towy Anticline ; north-west of that anticline there are several inliers while to the east of it is the Builth-Llandrindod inlier ; they are widely distributed in North Wales and reappear in the Borderland but are not known at or below the surface east of Shropshire.

There were important earth-movements at the close of the Cambrian. In Wales, the base of the Ordovician (Arenig) is unconformable upon Cambrian and Pre-Cambrian. This unconformity is strikingly demonstrated in north-west Wales ; at Tremadoc, the Arenig rests upon an eroded surface of Tremadoc while to the west at St. Tudwal's it transgresses from the Lingula Flags to the local equivalents of the Rhinog Grits, indicating that some 6,000 feet of strata were removed ; finally, west of St. Tudwal's, Arenig rests directly upon Pre-Cambrian. Similar relations are seen north of the Snowdonian Syncline, where the upper part of the Lingula Flags and the Tremadoc are missing (although this may be due in places to faulting), while a few miles away in Anglesey Arenig is once more upon Pre-Cambrian. There must have been considerable uplift whereby Cambrian sediments laid down along the southern margin of the Irish Sea land-mass were partially removed and if, as seems likely, they were deposited in Western Lleyn and Anglesey, wholly removed.

The Arenig is also unconformable upon different horizons around the Harlech Dome

while on its south-eastern flank, at Rhobell Fawr, there was volcanic activity in the Cambro-Ordovician interval ; the Trefgarn volcanic rocks in Pembrokeshire may possibly belong to the same episode and in South Wales as, for example, on Ramsay Island, Arenig is unconformable upon Lingula Flags.

In the Borderland there may be a break between Arenig and Upper Cambrian in the Shelve country but there were clearly very important movements in the Midlands where Cambrian seas had spread far and wide. No Lower and Middle Ordovician rocks are known east of the Longmynd ; the Upper Ordovician (Bala) only extends eastwards for a short distance and its characters indicate an approach to a shore-line ; the country east of Shropshire was evidently land during the Ordovician. It seems doubtful whether sedimentation was continuous anywhere from Cambrian into Ordovician times in the Welsh-Midlands region but there is a broad belt between the Harlech Dome and Shropshire where the base of the Ordovician is far below the surface and its relations unknown.

These movements defined the limits of the Welsh geosyncline, the margins of which during the Arenig lay in Anglesey and in Shropshire with material being derived from each side. These general conditions with some changes in the position of the margins persisted throughout the Ordovician, the Bala rocks of Central and North Wales in particular revealing a westerly derivation of material. The Irish Sea–St. George's Channel area was geanticlinal, while to the east of Shropshire lay the main land-mass on that side of the trough. There was considerable volcanic activity ; in South Wales and Merionethshire during the Arenig and Llanvirn and in the latter period in the Borderland. Vulcanicity ceased in South Wales at the beginning of the Llandeilo but continued in Merionethshire and commenced in Snowdonia, continuing in the latter area during the early Bala. By the close of the Ordovician, some 15,000 feet of sedimentary and volcanic rocks had accumulated in certain places within the geosyncline.

O. T. Jones and W. J. Pugh have been working for the past ten years on the Builth-Llandrindod inlier, the general succession, particularly in the sedimentary rocks, having been described by G. L. Elles (1939). This inlier, which covers about 40 square miles, includes a varied assemblage of sedimentary, extrusive and intrusive rocks ; the key to its remarkable geological structure and history was only obtained by detailed, large-scale mapping. We published a preliminary account in 1941. The succession begins with the Lower Llanvirn, *Didymograptus bifidus* zone, dark, flaggy shales with sporadic bands of ash ; they are succeeded, probably unconformably, by a group of rhyolitic ashes which are overlain unconformably by the Builth Volcanic Series belonging to the *D. murchisoni* zone.

The Builth Volcanic Series, about 2,000 feet thick at Builth, can be divided into eight groups and includes spilitic and keratophyric rocks ; the spilites were previously mapped as intrusive, but their extrusive origin is clearly revealed in the large quarries at Builth. The lateral variations in these rocks have been traced and, as a whole, they appear to have been derived from a centre, now concealed beneath Silurian, south of Builth. They are overlain unconformably by the Newmead Series, about 500 feet thick, consisting of felspar sands with boulder-beds, and flinty mudstones. These rocks also belong to the *D. murchisoni* zone.

They are succeeded unconformably by the Llandeilo, consisting of dark shales and flags. belonging to the *Glyptograptus teretiusculus* zone, followed by similar sediments belonging to the *Nemagraptus gracilis* zone ; the latter are the highest rocks seen in the inlier, which is everywhere surrounded by unconformable Silurian.

The phenomena which accompany the unconformity between the Newmead and Builth Volcanic Series are unique in Lower Palaeozoic stratigraphy (O. T. Jones and W. J. Pugh, 1949). The Newmead sands and boulder-beds are beach deposits and the contact between them and the Builth Series reveals the characteristic features of a shore-line with wave-worn surfaces, sea-cliffs, stacks and screes. The sands which contain marine fossils descend into cracks and fissures in the underlying rocks and in one place an ancient cliff of spilite surmounted by sand and with sand adhering to its vertical face must have remained virtually unchanged since *Didymograptus murchisoni* times. It is possible to reconstruct in detail the features of the shore-line and of the land adjacent to it, eventually submerged beneath Newmead sediments. It is an astonishing chance of preservation and of present-day erosion level that a panorama of an Upper Llanvirn shore-line can be drawn with complete confidence and that the details of it can be demonstrated on the ground without ambiguity.

The Builth Volcanic Series was accumulated on the sea-floor and the upper part of it was elevated to a height of about 700 feet at Builth ; this uplifted area was sub-aerially eroded, the horizontal spilites making a terraced topography which was capped by rounded hills of keratophyric rocks. As submarine erosion proceeded the area gradually subsided and was buried beneath Newmead and later deposits. It is remarkable that all

these events took place within a very restricted part of the time-interval represented by the *D. murchisoni* zone while, at a much later date, the rocks were tilted to their present westerly dip of about 30°. The uplifted tract, of which a portion of its eastern margin is seen to-day, lay well to the west of the main Llanvirn coast-line.

This work appears to raise interesting questions concerning the Llanvirn-Llandeilo in other parts of South Wales and possibly elsewhere, for example, in the Shelve area of the Borderland. The succession at Fishguard in the Fishguard Volcanic Series, A. H. Cox (1948), recalls that in the Builth and Newmead Series, although the rocks have there been assigned to the Lower Llanvirn. The Fishguard volcanics have been described by W. D. Evans (1945) in the Prescelly Hills, but the spilites are absent ; his map appears to indicate that the series rests unconformably upon the *D. bifidus* beds while the transgression of the Llandeilo across the *D. murchisoni* zone also recalls the relationships at Builth. It seems possible that the Llanvirn-Llandeilo rocks of South Wales may reveal a very varied and remarkable history.

The Ordovician of the Builth-Llandrindod inlier includes a very large number of intrusions with many features of interest ; so far, only the dolerites have been described (O. T. Jones and W. J. Pugh, 1946, 1948, 1949). The dolerites are found in the dark shales and flags ranging from the *D. bifidus* beds to the *Dicranograptus* beds, the latter exposed outside the inlier. There is reason to believe that they were injected in pre-Upper Bala times and that the sediments were horizontal at the time of intrusion. The intrusions at lower stratigraphical levels are sills but they become more and more discontinuous at higher levels and assume a laccolithic habit.

The Llandrindod Complex (1948) in the *Nemagraptus gracilis* shales was originally mapped as a continuous dolerite mass of roughly triangular form, the base of the triangle being nearly two miles long and conforming to the strike of the country rocks which dip north-westwards ; the widest part is some three-quarters of a mile across. Detailed mapping reveals that in fact it consists of more than 300 separate dolerite masses, the envelope enclosing them corresponding very closely with the boundary as previously recorded but only 25 per cent. of that area is occupied by dolerite. Ovoid masses in quarry sections are seen to be small laccoliths fed by stalk-like bodies at right-angles to their base ; they are the first examples where the stalks of these mushroom-shaped intrusions have ever been seen. There are about 100 small plug-like masses in the complex ; these were probably feeders to larger masses above them which have been removed by erosion. The Llandrindod Complex is a multi-layered intrusion, the dolerite masses occurring discontinuously on successive stratigraphical levels but extending less distance laterally as one ascends in the succession. The envelope enclosing the complex resembles the form of a laccolith but only 25 per cent. of the arching can be attributed to the igneous rock, the greater height of the structure in the centre being due to the greater thickness of shales into which dolerites have been intruded.

The Welfield Complex (1946) is a smaller but similar layered structure at a lower stratigraphical level in the *Glyptograptus teretiusculus* shales. There is no direct evidence of the structure of the inner core but the present surface is probably a random section through its outer skin. Dolerites occupy about 50 per cent. of the area within the envelope and it seems reasonable to infer that the arrangement of dolerite and shale within the structure is broadly similar to that now seen at the surface.

We have discussed the factors which have controlled the injection of these dolerites (1948) and have compared them with those in the classic region of the Colorado Plateau in the western part of the United States (1949). We have suggested that a transition may be found between Gilbert's ideal form and a much divided structure like that at Llandrindod, the underlying unity of the types between the two extremes being the form of the space within which the igneous rock is included and that is determined by the incidence of feeders. It seems probable that the internal structure, whether wholly or partly igneous rock, may be largely determined by the depth of cover, although the detailed arrangement of the intrusive masses in those which are much divided may depend upon other factors ; the Llandrindod Complex was probably intruded beneath a small cover into wet muds. The Builth-Llandrindod inlier demonstrates very clearly that igneous activity is closely connected with tectonic development and therefore intimately related to geological history.

Lake District

The succession in the Lake District, with which may be included the Lower Palaeozoic inliers to the east and south-east, begins with the Skiddaw Slates, the base of which is not seen. These rocks, which may be as much as 12,000 feet thick, are of geosynclinal character but were deposited in water of no great depth. They belong to the Arenig and Lower Llanvirn and are succeeded by the Borrowdale Volcanic Series, probably about 10,000 feet thick, and consisting of lavas and

pyroclastics. Recent detailed studies of the stratigraphy and structure of these rocks are those by G. H. Mitchell (1940), who has continued his work around Coniston, and J. J. Hartley (1941), who has extended his about Helvellyn. There is much variation in the characters of these rocks as they are traced from place to place and they may be mainly of Llanvirn age. The Bala rocks, relatively shallow water sediments, rest unconformably upon Borrowdales which were folded and eroded in pre-Bala times.

Scotland

Ordovician rocks are widely distributed in the Southern Uplands and with them may be included those at Girvan. The Arenig belongs to the graptolitic facies and includes spilitic lavas. It seems certain that there are no Llanvirn and Llandeilo sediments and if they were deposited they were uplifted and removed before the deposition of the Bala, which exhibits a graptolitic facies to the east and a shelly facies to the west.

J. G. C. Anderson (1947) compares certain black shales with basic lavas, the latter, where determinable, belonging to the spilitic suite, with Arenig rocks in Ayrshire and elsewhere. They are overlain unconformably between Aberfoyle and Loch Lomond by a coarse breccia succeeded by grits, the latter distinguishable from the Psammitic Group of the uppermost Dalradian because they contain fragments of black shale and chert ; these are compared with the greywackés of the Southern Uplands and, although their age is uncertain, they may be Bala.

The Durness Limestone in the North-West Highlands, as previously mentioned, must be referred mainly to the Ordovician, Arenig and, maybe, later series.

General

The Ordovician rocks of the Lake District are on the whole more like those of Wales than the Southern Uplands ; in particular, the faunal relations of the Bala are with Wales rather than with Girvan, suggesting a barrier between the Lake District and Southern Scotland. It seems reasonable to suppose that the Lake District rocks were deposited in the north-eastward continuation of the Welsh geosyncline and that the sediments were derived from a geanticlinal area in the Irish Sea and from a continuation of the Midlands land-mass into the north of England, the relative proportions from each flank varying from time to time. We may note again that important movements took place in this part of the geosyncline in pre-Bala times.

The lithological characters and thickness of the different divisions of the Durness Limestone are almost exactly the same along the whole length of outcrop, suggesting that the latter is parallel to the trend of the basin in which the rocks were deposited. The land, which presumably lay to the west, is supposed to have been of low relief and possibly under desert conditions, in order to explain the absence of terrigeneous material and the well-rounded sand-grains recorded in the limestones. While Durness Limestone conditions were prevailing in the North-West Highlands, clastic sediments, accompanied by submarine volcanic activity, were being deposited in the Southern Uplands, Girvan and probably the Highland Border, but we do not know whether these southern seas were in direct communication across central Scotland with the Durness basin.

In the Southern Uplands and probably in the Highland Border, there was post-Arenig, pre-Bala uplift and erosion. The distribution of the graptolitic and shelly facies of the Bala suggests land to the north of Girvan, possibly somewhere near the Highland Border, and the presence of a geosyncline between it and the Irish Sea barrier. This geosyncline continued south-westwards into Ireland and in it also may have been deposited the rocks of south-eastern Ireland.

If the Durness basin was in existence in Bala times, then it appears to have been separated from the Girvan-Moffat trough by land over central Scotland, but it should be noted that limestones at or near the base of the Bala in southern Scotland show faunal affinities with North America, as does the Durness Limestone. It may be that central Scotland was uplifted by post-Arenig, pre-Bala earth-movements, but it is also to be remembered that sediments, probably of Arenig age, appear to be unconformable upon ' Dalradian ' Cambrian ; in Wales and in England (and in eastern North America) there were important post-Tremadoc, pre-Arenig movements and these may have affected the Scottish area. One is tempted to speculate whether the incomplete Cambrian succession in the North-West Highlands may be due to these movements and whether the area between the North-West Highlands and the Highland Border was one of uplift before the Ordovician period.

Silurian

The Silurian rocks are found in the same general areas as the Ordovician but extend eastwards into the English Midlands ; they are not known north of the Midland Valley of Scotland. They exhibit the shelly and graptolitic facies but the uppermost sediments (Downtonian) are transitional into the non-marine deposits of the Old Red Sandstone ;

vulcanicity ceased except for slight localised outbursts at the beginning of the period.

Wales and the Welsh Borders

The rocks are seen in South Wales, south and east of the Towy Anticline ; they occupy most of Central Wales and extend northwards, eventually covering large areas in north-east Wales. They are seen in the Borderland and as far east as Staffordshire ; they are also known beneath the surface in south-east England.

Over most of Wales they appear to succeed the Ordovician conformably but there was a considerable change in the character of sedimentation. However, on the south side of the Berwyn Dome, Lower Llandovery rests upon Bala rocks well below their summit, while in the shelf-sea region Upper Llandovery is unconformable upon Ordovician, Cambrian and even Pre-Cambrian. The Ordovician rocks at Builth are broken by a complex and intricate system of faulting, most of which was the result of earth-movements which took place before the deposition of the Upper Llandovery which rests directly upon different levels in the Ordovician.

The shelly facies of the Lower and Middle Llandovery is found in the Haverfordwest-Llandovery belt and in the latter area has been described by O. T. Jones (1949). These sandy sediments are about 3,000 feet thick and are replaced westwards across the Towy Anticline by the graptolitic facies which, though at first thick, gradually diminishes in thickness north-westwards until at Conway, on the North Wales coast, it is only about 100 feet. These graptolitic rocks have been described in detail in many places and a recent account of them is that by W. D. V. Jones (1944) in the Llanidloes country in Central Wales. The sediments appear to have been derived mainly from the east and the shore-line presumably lay not far away from the Haverfordwest-Llandovery outcrop but grits in the Middle Llandovery south of Plynlimmon indicate on the other hand a westerly derivation from the St. George's Channel area.

The characteristic feature of the Upper Llandovery was the development of widespread shelf-seas east and south-east of the geosyncline in which the shelly sediments from Shropshire to the Mendips and south of the Pembrokeshire coalfield were deposited ; littoral conditions prevailed in these areas. The shelly facies along the margin of the geosyncline is again well developed in the Haverfordwest-Llandovery belt where it may be 1,500 feet thick. The change of facies at the margin has been demonstrated by O. T. Jones (1947) near Builth, where a thin development of Upper Llandovery, unconformable upon Llandeilo, is replaced in less than three miles by a thicker and partly graptolitic succession. Graptolitic rocks occupy most of Central Wales and include a considerable thickness of greywackés, the Talerddig Grits, the Aberystwyth Grits, the Cwmystwyth Grits, up to 8,000 feet or more in thickness. These rocks thicken to the west and disappear to the east, indicating derivation from the St. George's Channel area. The Upper Llandovery rocks thin rapidly northwards and at Conway are only 200 feet thick. In many parts of Wales (and in Shropshire) the highest beds consist of purple and green shales, deposited probably under shallow, lagoonal conditions ; these remarkable rocks are finely displayed at Llanidloes (W. D. V. Jones). The unconformities which separate the three stages of the Llandovery in the Haverfordwest-Llandovery belt have been attributed to folding at right angles to the margin of the geosyncline ; this cross-folding did not normally continue north-westwards into the graptolitic facies but at Rhayader there is a conspicuous unconformity at the base of the Upper Llandovery.

The Wenlock rocks of Shropshire and the areas to the south and east belong to the shelly, shale-limestone facies typically developed in Wenlock Edge and Woolhope ; a detailed account of the Wenlock Limestone and its reef-structures in the Dudley inlier has been given by A. J. Butler (1939). The limestones disappear westwards on passing to the graptolitic shale facies at Builth and elsewhere. O. T. Jones (1947) has confirmed the succession of graptolite zones at Builth and recorded slump-beds in the upper part of the succession, the first record of such phenomena in the Wenlock rocks. He has also shown that the Wenlock rocks at Builth are situated on an important fracture-belt which probably links up south-westwards with the Towy Disturbance and may be continuous north-eastwards with that recorded by J. R. Earp (1940) in Clun Forest. This fracture-belt, evidently an important element in the structure of this part of Wales, is roughly parallel to and about eight miles west of the well-known Church Stretton Fault-Belt. The shale facies is distinguished by the incoming of coarse grits north of Llandrindod and this grit-shale facies extends into North Wales ; these sediments were evidently derived from the west and northwest.

The Ludlow was characterised by similar physiographical conditions. The shale-limestone facies of Shropshire and elsewhere passes westwards into the grey, sandy mudstones and grits described at Builth by S. H. Straw (1937) and in Clun Forest by J. R. Earp (1938, 1940). The rocks at Builth are

5,000 feet thick and, except for a few hundred feet at the base, are characterised by shelly fossils. The Ludlow rocks occupy extensive areas in North Wales where, with the Wenlock, they have been studied by P. G. H. Boswell, recent papers being Boswell and Double (1940) and Boswell (1943). They have been classified by means of graptolites but at certain levels there are shelly fossils which may prove useful for correlation with Builth and Clun Forest. The thickness is about 5,000 feet, but Upper Ludlow rocks are not seen, although their former presence is attested by fossiliferous Upper Ludlow fragments in the Carboniferous basement beds. It is evident that this great thickness of sandy mudstones must have been derived from the west and north-west ; and, like the Wenlock, they indicate widespread shallow water conditions.

The Ludlow of Wales contains the finest development of slump-beds which are known in Great Britain. They have been described in North Wales by O. T. Jones (1937, 1939), at Builth by S. H. Straw (1937) and in Clun Forest by J. R. Earp (1938, 1940). The Ludlow rocks consist normally of well-bedded, often laminated, sediments but amongst them are layers of similar sedimentary material which is disturbed and often contorted in an extremely complex fashion. These latter are the slump-beds and the structures in them are clearly not of tectonic origin ; there is no evidence of faulting or thrusting at the contacts between disturbed and undisturbed layers ; moreover, the regional structure of these Ludlow rocks shows no evidence of close compression. These disturbed sediments were formed as the result of submarine sliding or slumping of deposits on the sea-floor and it is clear that the sliding of an individual layer was completed before the deposition of the undisturbed beds which overlie it ; the upper contact between disturbed and undisturbed sediments is a depositional one.

These disturbed rocks can be readily mapped ; they form lenticular masses varying in thickness and in lateral extent. They are thus of value in mapping rocks of the Ludlow type which in Wales are somewhat monotonous in lithology and in which fossils may be rather sparsely distributed throughout a considerable thickness of sediment. They also afford evidence, if the direction of sliding can be ascertained, of the original slope of the sea-floor and possibly even of its gradient, although it is known that subaqueous sliding of sediment may take place on very gentle gradients. They are therefore useful in inferring the form and distribution of the basins in which they accumulated and afford evidence of physiographical conditions. O. T. Jones (1938) has suggested that in Denbighshire the direction of movement in the disturbed beds was in general to the south, which might be expected since the Ludlow sediments as a whole were derived from the north-west ; those at Builth appear to have moved to the west but the direction of movement is as yet uncertain in Clun Forest.

The Downtonian rocks are typically developed in Shropshire ; they are known elsewhere in the Borderland and extend eastwards into Staffordshire. They are also found on the eastern flank of the Towy Anticline but except for an incomplete development in Clun Forest are not known elsewhere in Wales. They were deposited probably in basins and lagoons where at times conditions approached continental. In Shropshire and elsewhere there appears to be a gradual upward transition into the Old Red Sandstone but east of the Towy Anticline the Downtonian is unconformable upon the underlying rocks.

Lake District

There is again, as in Wales, a marked change in the characters of the sediments in passing into the Silurian. The Llandovery is a thin graptolitic facies like that in North Wales and of approximately the same thickness ; it would appear that the eastern shoreline was not far away from the Austwick area. The Wenlock consists of mudstones and grits, belonging to a graptolitic facies but deposited in water of moderate depth ; again, there is some evidence of shore-line conditions east of Austwick. The Ludlow is a great group of sediments, often of coarse texture, and may be as much as 12,000 feet thick ; they belong to a graptolitic facies but there are some shelly fossils ; they too were deposited in quite shallow water. No Downtonian rocks are known in the Lake District.

Scotland

The Lower and Middle Llandovery of Moffat consist of a thin, graptolitic development, 70 or 80 feet thick, but these rocks expand in thickness north-westwards across the strike passing into sandy shales and grits, whilst on the other side of the Ordovician belt at Girvan the equivalent rocks are coarse-grained sediments with shelly fossils.

The Upper Llandovery is 3,000 or 4,000 feet thick and consists of mudstones with bands of grit and conglomerate ; these rocks are widely distributed in the Southern Uplands, are mainly graptolitic, and the green and purple beds at the top recall the rocks at that level in Wales. It seems unlikely that the relatively coarse-grained Upper Llandovery could have been transported through

the Girvan area where a mainly graptolitic or fine mudstone facies was at that time being deposited, the true shelly facies of the Upper Llandovery not being well represented on the north-western side of the Girvan-Moffat geosyncline.

Wenlock rocks, consisting of conglomerates, grits and shales with both graptolites and shelly fossils, are seen on the southern flank of the Southern Uplands, but they may be incomplete. The equivalent rocks at Girvan contain less coarse sediment, while those in the Pentland Hills include calcareous mudstones with a fairly prolific shelly fauna ; A. Lamont (1947, 1948) has recently brought forward evidence to show that much of the so-called Wenlock of the Pentland Hills probably belongs to the Upper Llandovery. Much of the Wenlock in the Southern Uplands was probably derived from an easterly direction or possibly from the north-east.

No Ludlow rocks are known in the Southern Uplands but they are said to occur in several inliers along the south side of the Midland Valley ; in the Wenlock and Ludlow of the Midland Valley and also possibly in the Upper Llandovery, there are at certain levels greenish and reddish shales with eurypterids and other fossils, indicating that conditions prevailed at times comparable with those of the Downtonian.

Downtonian rocks are only recorded in the inliers on the south side of the Midland Valley and at Stonehaven, where they rest unconformably upon pre-Silurian strata and pass upwards conformably into the Old Red Sandstone.

General

The Welsh geosyncline was maintained during the Silurian but beginning with the Upper Llandovery and continuing throughout the Wenlock and Ludlow the sea spread eastwards and southwards over areas which had been land during the Ordovician. There is clear evidence of the westerly derivation of material from the St. George's Channel–Irish Sea region ; in Central Wales during the Middle and Upper Llandovery, whilst in North Wales it is abundantly demonstrated by the Wenlock and Ludlow, during which periods there were widespread shallow-water conditions.

The Welsh geosyncline extended northeastwards into the Lake District and the sediments were probably derived from both sides of the trough, on the west from the Irish Sea geanticline and on the east from the stable area in the north of England, although there is no evidence that the sea transgressed widely over the latter as it did to the east and south of the Welsh region.

The Girvan-Moffat geosyncline persisted and there is evidence of an easterly derivation of material during the Upper Llandovery and Wenlock, suggesting separation from the Welsh-Lake District trough ; the Ludlow rocks are only found in the Midland Valley of Scotland and it is not known whether they were deposited in the Southern Uplands.

There are no Downtonian rocks in North Wales, the Lake District and the Southern Uplands ; these rocks are restricted to the south-eastern and north-western flanks of the Silurian tract. It may be that they were never deposited in the central region because it was already being uplifted and eroded as a result of the onset of the Caledonian orogeny which closed the Lower Palaeozoic cycle of sedimentation.

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SECTION D.—ZOOLOGY

ZOOLOGY OUTSIDE THE LABORATORY

ADDRESS BY
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PRESIDENT OF THE SECTION

ZOOLOGY is partly science and partly natural history; that is if by natural history we mean the qualitative description of nature, and by science the quantitative and experimental analysis of its interactions. Zoology, by these definitions, is becoming more and more scientific; we do well to applaud the advance of exactitude, but let us not underrate the contributions of the naturalists who, in the spirit of explorers, have revealed for us the marvellous multiplicity of living things and by their descriptions have given us the facts both of their structures and habits. Discoveries by observation may be just as fundamental as those made by the experimental method. We are apt perhaps to forget that in the make-up of Chemistry and even Physics, we find plenty of descriptive natural history in addition to quantitative science: that for example dealing with the properties of the elements and compounds, and the colours of the rainbow. Opinion may well differ as to the propriety of defining science in this way, but it is convenient for my purpose to do so in order to make a simple distinction between the quantitative or experimental and the purely descriptive aspects of our subject.

The period of the great traveller naturalists reached its zenith in the middle of the last century: with Darwin, Wallace, Hooker, Bates, Trimen, Fritz Müller, Mosley, Agassiz and many more. I heard someone recently contrasting the outlook of T. H. Huxley with that of a field naturalist, as if the great Thomas Henry had never been in the field himself; his voyage on the *Rattlesnake* was in fact only one year shorter than Darwin's five years on the *Beagle*. It was of course this grand phase of natural history which gave the world the greatest generalisation Biology has yet made: the concept of Evolution. The stimulus of "The Origin" then sent the majority of zoologists into the laboratory to seek in the detailed study of comparative anatomy and embryology the clues of relationship between the different animal groups and so gradually to try to reveal the likely course of evolution. Last year our President referred in his address to the revolt, after the first world war, of the younger zoologists against this purely morphological trend and to the rapid advance in experimental work which followed. For more than half a century our Zoology has been mainly in the laboratory. We are now seeing signs of a reaction: a going out into the field again. It is this 'back to Nature' movement which I want to discuss in my address to-day, and I have called it 'Zoology outside the Laboratory.'

Some people seem to think that zoology in the field cannot be more than natural history. 'By all means,' they say, 'get your ideas for research from nature, but you must then bring your hypotheses and material into the laboratory to be tested.' Or as a colleague said to me the other day when we were discussing a project of ecological research: 'Of course ultimately it must end up on the laboratory bench.' Again I have heard it said 'Ecology only becomes really interesting when it turns out to be something else.' No doubt there is a great deal of sound sense in these remarks, but they are not always or entirely true. Certainly a great many hypotheses can only be tested in the laboratory; this must apply to nearly all problems whose solutions lie in the fields of physiology, biochemistry and micro-anatomy. Much of what is exciting in ecology does indeed reach its climax within these fields of laboratory biology; but not all the interest is indoors. In this address I want to discuss the development of zoological field studies and the reasons why I consider them to be so important at the present time. Let me however emphasise at the outset that I am not advocating more field work at the expense or neglect of laboratory zoology. The two sides of our science must be linked closely together; their segregation would be as lamentable as has been the divorce of morphology and physiology in the past.

The idea that science belongs to the laboratory and what lies outside is natural history, springs I think from the belief that all biology will ultimately be explained in terms of physics and chemistry: that physiology,

biochemistry and bio-physics are the scientific core of zoology. These indeed do at present appear to make up the science of the organism as analysed from within ; but just as important is the science of the animal as a whole looked at from the outside and in relation to its natural surroundings : the ecological and ethological aspects of biology. It is true that much of what is called ecology is really natural history carried out by those who seem to feel that their pursuit will be thought old-fashioned if it is not given a more modern name. Natural history is nothing to be ashamed of ; it is the necessary pioneer work in preparation for the experimental and statistical analysis that must follow in Ecology.

When we record that a particular kind of fish is confined to warm oceanic water and feeds upon various kinds of shrimps, that is simple natural history. When we can determine the temperatures and salinities which limit its distribution and by a vast number of post-mortem examinations work out the average percentage constituents of its food at different times of the year and for fish of different ages, we are then beginning to know a little of its ecology. Ecology, according to our definitions, is the conversion of natural history into science. But its aim is not simply to express the inter-relationships of organisms with their environment, living and inanimate, in numerical terms ; that in itself would be a tedious refinement. It is from this quantitative analysis that it hopes in time, step by step, to discover more of the laws operating in the world of living things. Experiment will come to play as big a part outside the laboratory as inside it, as the students of animal behaviour are so brilliantly showing. Just as physics and chemistry are based upon statistical laws concerning the behaviour of electrons, atoms and molecules, so ecology is science *in its own right* based upon the statistical treatment of the interactions of organisms as living wholes. Ecology could exist as a true science if the laws of physics and chemistry had not yet been discovered, although it would be a sadly limited science. Any one branch of science is handicapped without support from other branches ; our ecology will be so much richer in the future when it gets more help from the rapidly developing studies of animal behaviour.

It is rather the custom for a President in his address to Section D to make some reference to his own field of work in zoology. It will then perhaps not be out of place if, as an introduction to what I want to say later, I sketch some little examples of ecology in action as I have seen it. Nearly all my research has been concerned with work outside the laboratory. The laboratory for me has been more of an office ; for beyond the work of identifying the specimens collected, it has been mainly a counting house where the estimations, statistics and tabulations of all that was done outside it were carried out. My experience of Ecology has been mainly in the sea.

At first sight it may seem strange that animal ecology should have advanced much further in the sea than on the land. In the ocean we are faced with two difficulties we do not usually meet with ashore. Firstly we are working in three dimensions. In most cases, forests being an exception, the land ecologist can think of his domain in plan ; the extensions of life into the atmosphere and underground are slight, so that he need usually only consider the distribution of his animals in relation to the earth's surface and can plot them on a two-dimensional map. In the sea we must take into account not only the horizontal movements of animals, but also the extensive vertical ones they often make. We must remember too that the environment itself is not stationary ; in relation to the sea bed the water is always on the move and not all of it over any one spot is moving at the same speed or always even in the same direction. The surface layers tend to move faster than the deeper ones and not infrequently there may be a reversal of movement : say a northerly current at the top and a return southerly current at the bottom ; and some animals may frequently make oscillatory migrations between them. Water movements may also have a vertical component : here may be an upwelling of water when an ocean current meets a submarine bank, or where a cooler surface current may meet a warmer and less dense water-mass and dip down below it. It is this moving environment which of course carries the basic food supply for all life in the sea. Here the phytoplankton, the innumerable specks of floating plant life, form the food of the hosts of planktonic animals which in turn feed pelagic fish such as herring, sprat or mackerel ; all the life of the sea bed too is dependent on the falling rain of material, living and dead, from the drifting community above it. In the sea the continuous movement of the environment must always be kept in mind. What would the land ecologist say if his woods and meadows changed position overnight ?

The second difficulty which faces the pelagic ecologist is more obvious and more formidable than the first : he is working entirely in the dark. Practically everything he is studying is hidden from him below an opaque and often none too friendly surface. I often wonder what progress we should have made in terrestrial ecology, or even terrestrial

natural history if the tables had been turned : if man normally lived a floating life on ships or rafts and the land was always covered with a thick asphixiating fog into which he could only penetrate a very little way and with the greatest difficulty. Man would certainly attempt to study its ecology ; he would attack it no doubt with rockets to carry his folded but suddenly expandable collecting nets as far inland as possible to be dragged back to the shore with their specimens. Nets on balloons would be sent drifting into the fog and pulled out again to catch the vaporial plankton. Gradually he could build up some picture of life in the hidden world. This is only a slightly exaggerated sketch of what the marine ecologist is doing ; the one real difference is that he has gravity to help him. He is a marionette artist dangling his nets and apparatus below him on strings to explore a stage he cannot see ; and bit by bit he builds up a picture of the drama being enacted upon it. He can perform experiments out of sight down below by pulling strings to operate a lever or sending messenger weights sliding down a wire to hit a trigger and so initiate some action. In this way a quantitative study can be made of the behaviour of vertically migrating animals placed in an apparatus and experimented with perhaps a hundred metres below the surface.

It does at first seem extraordinary that the ecology of this hidden world should be so much more advanced than that of our own surroundings. Only just now are a few ecologists beginning to put the study of the food and feeding habits of some of our common birds and mammals on a truly quantitative basis ; at present we know far more about the food relations of a number of marine fish than we do about any terrestrial animals. We have a better idea of the populations, per unit area, of different molluscs on many parts of the sea floor than we have of the snails of our countryside ; our knowledge of the numbers of different small crustacea per cubic metre of water in many seas of the world is far in advance of such information regarding the insect numbers on land. I think it possible that we know more about the ecology of the Dogger Bank and its overlying waters, than we do about the ecology of Northumberland or Durham.

In the 1890's Victor Hensen and the Kiel planktologists had developed their special methods for estimating the numbers of different plankton organisms per cubic metre of water and were also attempting to get a relative idea of the strengths of the stocks of plaice in different years from a measure of the average number of plaice eggs found floating per unit volume of sea. By the turn of the century C. G. J. Petersen was beginning his quantitative sampling of the animal life of the sea bed by using a specially devised grab-dredge and was working out the different communities over the floor of the Baltic. A study of the periodic fluctuations in the fish populations was well underway before the first world war, and already by then the extensive liberation of marked fish for recapture by fishermen had told us much about the migrations, growth-rates, and actual size of the stocks of the North Sea Plaice.

I'm afraid we cannot claim that this advance is explained by any greater enthusiasm of the marine ecologist ; we must admit that it was due to the stimulus of commercial needs. Man lives mainly on cultivated crops, on the flesh of domesticated mammals and upon fish. In the same category as the fish we may put the whales which give us so much oil for margarine ; fish and whales are alone unfarmed and still hunted in the wild. It is of course the economic urge to find out more and more about them, so as to make their exploitation more efficient, that has pushed forward this new science of ecology below the surface of the sea.

For a long time we have had our marine laboratories : Naples, Plymouth, Millport, Woods Hole and the rest, not to forget the excellent little Dove Laboratory at Cullercoats ; they have mostly been concerned with true laboratory zoology, doing fundamental work on the anatomy, embryology, physiology and biochemistry of the various organisms brought in from the sea to be examined on the bench. But apart from these, there has arisen since the beginning of the century a vastly greater enterprise in the marine zoology that lies outside the laboratory. Almost every nation on the seaboard of Europe has had its fishery research vessels working incessently, except for the periods of the two wars, to build up a picture of life in the sea expressed in quantitative terms. Not only are they studying the fluctuations in the populations of adult fish, or of their eggs or larvae in different years or the variations in their growth rates or the paths of their migrations and so on, but also they are studying just as intensively the varying quantities of fish food available in the plankton or on the sea bed and the changes season by season in the physical and chemical background. In 1925 began the great series of ecological voyages to the Antarctic seas at first under the leadership of Dr. Stanley Kemp, whose untimely death during the war we have all felt so deeply. The work of these Discovery expeditions is exactly similar to the northern fishery work—the ecology of the whole marine community—but centred round the biology of whales rather than of

fish. Already we know a great deal, not only of the natural history, but of the ecology of the Southern Ocean ; in fact probably more than about any other waters of the world except those immediately round our islands. Let us rejoice that these investigations are to be continued, now linked with the new National Institute of Oceanography. The establishment of this new Institute this year is an event of the first magnitude in the history of British Science and one of special interest to our section ; it is to be concerned as much with the life of the ocean as with its physical and chemical properties, and its sphere of action is the very globe itself.

It has been my privilege and good fortune to have taken part in both of these great adventures in zoology afloat : on the North Sea and in the far south. Let me indulge in a few personal reminiscences which have particularly impressed me and which I think will help to illustrate ecology in action. I will first recall an experience when serving in that grand old research trawler, the *George Bligh*, of the Lowestoft Fisheries Laboratory of the Ministry of Agriculture and Fisheries. In March 1922, through the illness of a senior, I found myself at the last moment of sailing, as naturalist in charge of the cruise ; I was then a young recruit in my first year of fisheries service, eager with enthusiasm to win my spurs. Our task was to steam backwards and forwards making a series of parallel tracks across the southern North Sea and the mouth of the English Channel, stopping at intervals of twenty miles to take observations and samples. Essentially we were making a survey of the distribution of the very young herring (larval and post-larval stages) and their planktonic food over this area. At each station (*i.e.* position at which we stopped) we took the temperature of the water and samples for salinity determinations at different depths, then steamed slowly forward towing three plankton nets at different levels : one at the surface, one in midwater and the third near the bottom and finally made hauls at these same three levels with the larger Petersen young fish trawl. Exactly the same routine was followed at each station. As was always the practice we steamed along our course day and night, coming upon our stations, which were separated in time by about 2½ hours, at any hour out of the twenty-four. The weather was unusually kind so that we had no delays and I found I could finish the cruise in six days instead of the seven allowed. I was at the time making a special study of the food and feeding habits of the young herring and, as they were so plentiful at one station, I decided to spend a day in one place devising methods of capturing them unhurt, so as to watch them feeding upon plankton in tanks on deck. I also thought that at the same time I would repeat the whole routine of the station at four different times during the twenty-four hours : partly as a check on the validity of the methods and partly to see if there was much difference between the vertical distribution of the young fish and plankton at different times of the day. We had arrived at this station and begun our first observations at 7 o'clock in the evening. I repeated the whole set of sampling again at 7 o'clock the next morning, at 2.30 in the afternoon and again at 7 o'clock the next evening.

During this time the ship at the surface had no doubt drifted slightly in relation to the main body of the water below and so we were sampling slightly different parts of it. The numbers of young (post-larval) herring taken respectively at the four times of sampling were 2,448, 24, 7 and 341. The range of difference between these numbers taken at the one station was greater than that between the largest and the smallest numbers taken at any of the other stations on the cruise. The variation in the total quantity of plankton other than young fish was considerable but not as great : the highest value being just about three times that of the lowest value. It showed how very patchy can be the distribution of both young fish and plankton at quite small distances apart. It showed also that the figures representing the numbers of young herring present at all the other stations of the cruise—and upon a number of similar previous cruises—were quite valueless. It was indeed a lesson. I cannot say that I returned a sadder and a wiser man ; I was certainly wiser, but to tell the truth a little jubilant. The cruise as such was a failure and I had hardly done something towards winning my spurs, but I had at least pricked a bubble of planktological confidence.

I realised that field ecology should be carried out in the same spirit as a laboratory investigation—each of our bits of work is really in the nature of an experiment. In this case the experiment was to expose nets in the water at intervals of 20 miles in order to find out the distribution of young herring over the whole area. A control experiment is necessary to see if such samples are repeated at the same place they give a reasonably consistent result : to see if in fact one such exposure of nets at a position can be said to give a measure of the plankton or young fish in that part of the sea. The control experiments I performed at the end should of course have been performed at the beginning before the cruise was undertaken ; one lives and learns.

I once heard a quite well known planktologist say that it did not do to arrange your

stations too close together because it made it almost impossible to use contour methods when charting the results ; I don't think he realised the significance of what he was saying.

It was this experience of the patchy nature of the plankton which led me to devise the continuous plankton recorder which can give a continuous line of ecological observation as it automatically samples the plankton on a moving gauze banding (graduated to represent units of distance) as it is towed across an area of sea. It gets over the problem of patchiness but of course it has its limitations in other directions ; if it is used at only one depth its results may be affected by the varying vertical migration of the different animals. That must be taken into account. The recorder does not supplant the tow-net for all classes of work ; its function is to make rapid broad surveys, especially those in which a number can be used at the same time. Much plankton work can only be done with nets or other allied sampling devices. An experimental and statistical study of the operation of plankton collecting gear has been long overdue and is now being carried out at the Millport Marine Station by Dr. Barnes.

Now let me take some other examples which I hope will help to restore confidence in ecological methods. Let us go to the other end of the world, to the South Georgia whaling grounds, where in 1926 and 1927 the old R.S.S. *Discovery* and the smaller R.R.S. *William Scoresby* together carried out an intensive survey of the plankton and hydrology round that island. My friend the late Mr. Rolfe Gunther, so tragically killed in the early days of the war, worked with me on the plankton and together we published the results. South Georgia, a long narrow mountainous island about a hundred miles in length, lies some 900 miles due east of Cape Horn ; it lies diagonally across the path of the westerly drift of the Southern Ocean at a point where water from the Bellingshausen Sea (in the extreme south of the Pacific Ocean) meets with water flowing out of the Weddell Sea, that great ice-covered gulf bitten out of the Antartic continent. Here is one of the places where the largest rorquals, the blue and fin whales, congregate in summer to feed upon the vast shoals of krill—the prawn-like Euphausiaceans (*Euphausia superba*) —which inhabit these seas in such amazing numbers. I must try to condense a long story into a few words. We surveyed the waters round South Georgia by a series of radiating lines running out from the coast to beyond the continental shelf ; the two ships took part together, working different parts of the survey at the same time, stopping every 15 miles to take samples and observations. At each such station standard methods were used : large coarse-mesh nets to catch the krill and small very fine-mesh nets to catch the microscopic plants—the phytoplankton —and of course the sampling of the seawater for analysis. Before the design of the survey repeated hauls with the plankton nets were made in series to test the patchiness of the more important forms and so determine the length of tow necessary at each station to give a reasonably valid result. At the same time as the survey was undertaken all the skippers of the whale-catchers operating in the district were asked to record the positions of the different whales they killed. We were thus able, at the end of the survey, to prepare a series of contour charts showing the varying distribution of the different items sampled: the whales, the krill, the phytoplankton, the temperature and the various chemical characters of the water such as general saltness, phosphates and nitrates, the oxygen content and its alkalinity (in terms of hydrogen-ion concentration). The distribution of the whales was correlated to a remarkable degree with that of their food, the krill ; the same thing had been found in an earlier survey the year before when the krill was much more concentrated against the coast, as also were the whales. Now the distribution of the krill in the main survey and that of the phytoplankton were inversely correlated ; where the krill were abundant the phytoplankton was scarce and *vice versa*. Whether the phytoplankton was scarce where the krill was because the krill had grazed it down or whether the krill was excluded from the zones of dense phytoplankton, according to a hypothesis I put forward, does not matter for the present argument. (This is the subject of a renewed investigation at the present time). Further the contoured maps of phosphate values gave a remarkably close inverse correlation with the phytoplankton distribution : where the little plants were more abundant, the phosphates in the water were more used up. The alkalinity (*p*H) contours also gave a good correlation : the water being more alkaline in the regions of dense plant production because of the carbon assimilation.

The stage is now set. From the phosphate chart we could deduce the phytoplankton distribution ; from the phytoplankton chart we could in turn deduce the distribution of the krill, and finally from that of the krill we could deduce the distribution of the whales. If this state of affairs was a usual one, then it seemed likely that we could predict the distribution of the whales direct from a knowledge of the phosphate values. One would expect whale concentrations only in regions where the phosphate values were relatively high. High

phosphates mean poor phytoplankton, poor phytoplankton means rich krill, and rich krill means many whales. Such a chain of inference is of no commercial significance in this particular case, for if one set out to chart the phosphate values to predict the whale distribution one would see the whales themselves blowing, if one kept one's eyes open, long before one could work out all the phosphate values and chart them. If however we can repeatedly show that the whale distribution is correlated with the phosphate distribution, then we will know that in our ecological methods we are getting to grips with reality in nature. After we returned from the 1925–27 expedition on the old *Discovery* and I had left to become a professor at Hull, the new R.R.S. *Discovery II*, or sometimes the little *William Scoresby*, again visited the South Georgia whaling grounds in several seasons to survey the plankton and hydrological conditions, and as before the whalers kept records of the position of the whales they killed. I should explain that the number of whales killed during most months of the season usually runs into many hundreds and so their pattern of distribution can be contoured like that of the plankton and phosphates. The plankton had not yet been worked out, but the phosphate values were soon determined and published. Charting these phosphate results for three further seasons one was able to say that from the correlations obtaining in the 1926 survey one would expect to find the whales in some areas and not in others. In each case the inference was correct. It was a pleasant experience for an ecologist sitting in a room at Hull to be able to work out correctly the main features of the distribution of whales at the other end of the world from a table of phosphate values. The exciting climax came when I considered the conditions at South Georgia in the season of 1930. In that year the *William Scoresby* had made a survey of the plankton but had not taken phosphate measurements. In this season the whale distribution turned out to be most exceptional, quite at the opposite side of the island to the more usual positions of concentration. The plankton had not yet been worked out but was stored in bottles in the British Museum. From the whale distribution I now forecast in broad outline what the plankton distribution should be like when it came to be worked out and asked Dr. Kemp if he would have it examined. He did and the distribution was just as expected.[1] As nothing else can do, prediction gives confidence in ecological method.

Confirmatory evidence from the results of two entirely independent workers also assures us that our methods are sufficiently adequate to give solutions at any rate to some of our problems. Let me take an example of this. In the summers of 1931, 1932 and 1933 we at Hull were making trials with a small instrument called the plankton indicator which was being used from herring drifters to collect samples of the plankton at the places where they fished. We were working out correlations between the catches of herring and the state of the plankton. The little hollow torpedo-shaped indicator was towed for a mile by each drifter just before the nets were put out for the night's fishing and the sample of plankton was obtained on a gauze disc which could be taken out of the indicator after use. These samples were then wrapped up and preserved in a formalin container and returned to us with a label giving the date, position and number of herring caught in the nets. In addition to the Scottish fisheries we were making an intensive study of the Shields fishery, off the Tyne. Quite independently at the same time Mr. Savage of the Ministry of Fisheries laboratory at Lowestoft was working on the food of the herring and obtaining preserved herring stomachs from the same fishery for analysis of their contents. In the late summer of 1931 the positive correlations, which we had been finding at other times and places, between the numbers of herring caught and the quantities of the favourite food in the plankton—the copepod crustacean Calanus—failed. The Calanus at this time had become exceedingly scarce and the herring were now feeding, as Mr. Savage showed, largely upon the arrow-worm Sagitta and a little pelagic mollusc, the pteropod Limacina. By comparing our results with those of Mr. Savage it was found that the average quantities of these two organisms upon our plankton indicator discs for a complete series of seven 10-day periods during July, August and September, bore an extraordinarily close relation to the average number of these organisms found by Mr. Savage in his herring stomachs. The two curves for each organism rose and fell together with an almost unbelievable correlation.[2] In the summer of 1932 and 1933 the herring were feeding largely on Calanus and it was abundant in the plankton. There was now a good correlation between the Calanus on the indicator discs and in the herring stomachs as found by Mr. Savage ; but no such correlation between Sagitta and Limacina. During these two summers also the correlations between the numbers of herring caught and the distribution of the Calanus in the plankton were markedly positive.

[1] See Discovery Reports, Vol. XI, pp. 276–301, 1935.

[2] See Hull Bulletins of Marine Ecology, Vol. I, p. 252, 1941.

Confirmation and prediction show that ecology is gaining the means to attack the problems before it and provide answers that will command our respect, or—less piously expressed—answers rendered respectable by the application of approved statistical tests.

We have seen that it has been the economic urge and the accompanying financial support that has pressed forward ecology in the sea so much further than on land, yet it has not pushed it entirely into grooves of applied research ; most fortunately the advisors of the governments supporting these investigations have been scientists who have realised that sound progress in the understanding of fishery problems can only be made through a knowledge of the whole complex web of life in the sea and its physical and chemical background.

If only cattle and sheep had never been domesticated and we were entirely dependent upon wild animals for our meat what a wonderful series of terrestrial ecological investigations we might have had by now to enable us to conserve the stocks of wild game and exploit them to the best advantage for both the present and the future. But isn't terrestrial ecology to-day really just as important as that—but not quite so obvious ? While the main sources of our food are cultivated plants and domesticated animals, they are attacked by a multitude of wild pests. We all know that, and also that there are now dotted about the world dozens of research stations. But most of these are either dealing specifically with the control of this or that pest, or dealing generally with all the different pests of this or that particular crop or domestic animal. Large sums of money are going into this, and rightly so, for the damage to our food from pests runs into millions. But how many research stations are there devoted to the science of wild life for its own sake ? How much money is being devoted to fundamental ecological research which has not the pretence or an excuse of an immediate economic end ? At present very few stations —if any (apart from University departments) —and relatively very little money. Yet it can only be through a better understanding of the basic principles underlying the complex interactions of wild life that really sound progress can be made in so many of the problems of applied research.

We have so much yet to learn of population dynamics : prey and predator, host and parasite relationships. The endless controversy on the economic status of different wild birds in relation to agriculture will continue —as Dr. Lack and Mr. Hartley recently pointed out in *The Times*—until much more quantitative work has been done to unravel the complex interrelationships between the birds, insects and the insects' parasites. Then there are likely to be many problems of pest and parasite which will only be completely solved by considerable advances in the study of animal behaviour—still in its infancy. There is still so much too to be learnt of the various factors governing reproductive rates and breeding under different climatic conditions. So much yet to be done in the great field of taxonomy.

But it is not the object of this address to bewail the fact that up to now fundamental research in terrestrial ecology has received so little financial support compared with applied biology, but rather to emphasise its importance and to sound a note of encouragement : to proclaim a conviction that zoology outside the laboratory is at last coming into its own.

Several things have happened in the last few years which I think may be regarded as significant of the coming change. In 1946, the Royal Society published a *Report on the Needs of Research in Fundamental Science after the War ;* in the forefront of their biological recommendations comes the following :

> ' With advance in biological knowledge it has become increasingly evident that fundamental research must be directed towards the study of living organisms and their relation with their natural surroundings. Such facilities as do exist for the study of living animals and plants under natural conditions deal almost only with marine and freshwater biology, and it is essential to develop opportunities for field research in terrestrial ecology in the widest sense.'

Secondly, I hope without boasting, but nevertheless with pride, may I refer to certain events which have occurred in my own University, Oxford. In July, 1947 a statute was passed setting up a new department in the Faculty of Biological Sciences : I believe the first in the world to have the title of The Department of Zoological Field Studies ; its objects are ' the study of animals in nature : their ecology, behaviour and evolution.' The particular significance of the event lies in the recognition of its status as a full university department ; actually it is new only in name, for it is formed by the bringing together of two earlier Oxford pioneer ventures. One is the Bureau of Animal Population under the direction of Mr. Charles Elton, whom we all regard as not only the founder of the Bureau but as one of the founders of terrestrial Animal Ecology itself. The other is the Edward Grey Institute of Field Ornithology founded as a memorial to Viscount Grey of Fallodon and now under the direction of Dr. David Lack. These two branches, each under its own Director, now form one interlocked

department which I am proud to say is in turn linked with the older Department of Zoology and Comparative Anatomy by being, like it, under my headship as Linacre Professor. In this way zoology outside and inside the laboratory are kept closely in touch. In the same year as this came about the University gave over the woodlands of the Wytham Estate to biological studies and established a field station there ; here last year Mr. Elton founded a students' vacation course in terrestrial ecology similar in scope to the long established vacation courses in marine biology at Plymouth and other coastal laboratories. Only a few months ago Cambridge announced the provision of a similar estate for such wild life studies. To bring my glad tidings from the Isis up-to-date I must record an event of great importance for Zoology in this country : Professor Tinbergen, formerly of Leiden University, is coming to Oxford next month to take up an appointment specially created for him : University Lecturer in Animal Behaviour.

Then we have the recent establishment of the various field centres under the Council for the Promotion of Field Studies which are providing such splendid facilities for visiting students, amateur and professional naturalists alike, in different parts of the country.

Now in the interval since the Association met last September has come about an event, the importance of which we can yet hardly realise ; to many of us it is like a Utopian dream come true. I refer of course to the establishment of the Nature Conservancy which has recently been placed under the Directorship of another great pioneer in terrestrial animal ecology : Captain Cyril Diver. It is formed for the conservation of the flora and fauna of Britain in general, but also for the maintenance of special nature reserves : areas of particular biological interest in different parts of the country. It means the saving for us of priceless bits of our national heritage, formerly threatened with destruction—bits of virgin forest, fen and moor ; it also means the provision of protected regions where field experiments can be conducted without the danger of interference.

As a last and still more up-to-date example of the turning tide in the fortunes of field zoology let us look at the programme of our section for the week in front of us ; it must be many years since we had so many different sides of zoology outside the laboratory represented, and mostly represented by special symposia. We appropriately begin with a talk on the north country naturalists : a band of great men whose achievements are built into the foundations for our later science. Then we have whole sessions on Marine Ecology, Agricultural Ecology and the Field Study Centres. Next we have a joint session with Physics and Engineering upon Aquatic Locomotion, a study which illustrates so beautifully the value of laboratory analysis and experiment in the understanding of the movements of animals in nature ; the same can be said of the study of the feeding of Amphioxus which is the subject of another paper. Then we have further sessions on the Biological Aspects of River Pollution (essentially an ecological problem), Insect Flight and Distribution, Salmon Fisheries, Bird Behaviour, and a final fitting and exciting climax : a session on field evolutionary studies.

I have stressed the necessity for more pure research in field zoology to provide a foundation for a better knowledge of the factors governing our food supply. That indeed is urgent, but to my mind such research is important for a much more fundamental reason. Man does not live by bread alone : not Man as most of us respect him. If he does come so to live—and it is a possibility, guided by a philosophy of materialism, either classical or dialectical—our civilisation will change. A little knowledge is certainly a dangerous thing. Who can doubt that the roots of the last two wars were not nourished to a large extent upon a philosophy of materialism and too small a knowledge of the working of evolution ? If we could spend upon fundamental biology half as much as we spend upon research on atomic energy, how much more we would know about the very nature of life and the process of evolution. The study of evolution in operation is one of the most exciting themes in ecology ; but here again it must be a joint enterprise between field and laboratory genetics, field and museum systematics, field and laboratory behaviour studies.

We reminded ourselves at the beginning that it was field natural history which gave us the conception of Evolution and, through Darwin and Wallace, the theory of Natural Selection. It was then in the breeding gardens that Mendel and later Johannsen, and in the laboratories that Morgan and his co-workers and so many more, showed us the true nature of variation and by giving us the laws of particulate genetics, swept away the great enigma of the older Darwinism : that of how selection could possibly work with the then supposed blending inheritance. Through the work in the laboratories, and in the studies of our mathematical geneticists, Darwinian selection is now firmly established: selection acting upon a gene-complex continually modified by recombination and mutation. It now seems likely that the genes (the units of heredity) are elaborate

molecules and that their change, their mutation, is due to a change in the pattern of the arrangement of their atoms ; in support of this we know that the rate of mutation can be increased by irradiating animals with X- or gamma-rays. These are all brilliant achievements of the laboratory ; but let us not be so dazzled by them that we expect and look for little more. It is less than a hundred years since the publication of Darwin's masterpiece and barely half a century since Mendel's laws were rediscovered ; is it likely that in this short time we have grasped more than a fraction of the whole truth of the evolutionary process ? It is surely by a fuller understanding of this mechanism that zoology, outside the laboratory—as well as within it—will make its greatest contribution to man's philosophy and to the shaping of his future civilisation.

There are a number of problems in evolution that can only be solved outside the laboratory. We want more evidence in quantitative terms of the actual operation of selection in nature. We want more studies on the lines of those pioneer observations of Professor Heslop Harrison on the selection of dark and light varieties of the Geometrid moth, *Oporabia autumnata* in pine and birch woods ; more experiments like those of Dr. Cecil Gordon in liberating a large population of Drosophila and by repeated trapping following the fate of a particular mutant gene. Dr. David Lack is giving us statistical evidence of the part played by natural selection in determining the size of families ; he is showing in birds how the tendency to greater productivity is balanced and then defeated by a greater proportionate mortality of the young in the larger families due to their malnutrition consequent upon the limit to the amount of food the parents can provide. Dr. E. B. Ford has for many years been studying selection in wild populations of butterflies and moths ; I hope that later in the meeting he will tell us something of the work he has been doing for a number of seasons on the Scilly Islands. Two years ago he and Dr. R. A. Fisher published an account of their remarkable investigation of the spread of a new mutant gene in an isolated colony of the Scarlet Tiger Moth *Panaxia dominula* near Oxford ; with their technique of capturing, marking and liberating—and recapturing, remarking and again liberating—they have been able by continually resampling to determine the size of the population and the percentage frequency of the mutant gene from year to year. This is possible because fortunately the heterozygotes can be readily distinguished. The relatively frequent heterozygous form *medionigra* has evidently some advantage and the very rare homozygous form *bimacula* is at a discount : a balanced polymorphism is established with the gene-frequency fluctuating in such a way that it must be under the influence of selection, although the whole colony in some years numbered no more than a thousand. The Sewell Wright effect of Genetic Drift, as indeed it's discoverer postulated, can only operate in isolated populations *much less* than a thousand strong.

Are such very small isolated populations in which genetic drift can give rise to non-adaptive variations really of importance in evolution ? Can they usually survive for long enough to provide in time the pre-adaptive forms which may take advantage of some new situation in the changing environment ? These are questions for field zoology to answer. Captain Cyril Diver has drawn attention to the pressing need for such investigations from his studies of small isolated colonies of snails of the genus Cepaea as well as of certain insects.

A still more important contribution that field zoology can make to evolutionary theory is to throw more light on the part played by Organic Selection. The gene combinations which are best suited to the *habits* of the animal may tend to survive in preference to those which do not give such full scope to the animal's pattern of behaviour. This idea of Organic Selection, which was put forward independently by Baldwin and Lloyd Morgan at the turn of the century, has been almost forgotten until quite recently. This possible selection of structural variations by habit as opposed to the selection of other variations by the environment may indeed be a factor of importance. It is in effect similar to that postulated by Lamarck but brought about on Darwinian lines. External Natural Selection must of course be important, but if Organic Selection can be shown to be a really significant factor, it may well alter our way of looking at evolution as a whole. The relative importance of the two forms of selection must be the subject both of experiment and of more research into the habits and behaviour of animals in nature.

The study of Animal Behaviour is another important field being opened up by zoologists outside the laboratory, using the experimental method ; it also has, I am sure, a great contribution to make to Man's philosophy.

Zoology in the laboratory is apt to lead towards a mechanistic view of life if it becomes divorced from the world of living reality outside it. No one in his senses will deny that the laws of physics and chemistry hold good within the animal body as outside it and that the body is, in its *physical* make-up, a machine. If we investigate it in the laboratory by physical and chemical means we can of course only get physical and chemical

answers. It is a much worn platitude to say that it is nonsense to consider the works of a Shakespeare, a Beethoven or a Rembrandt as the products of a machine ; but it is a platitude worth repeating. If man is a member of the animal kingdom as we all believe, his works of genius are a manifestation of organic behaviour. To proclaim that organic behaviour is the product of mechanism as we *ordinarily understand it* is to my mind as unreasonable and as dogmatic as to proclaim the literal interpretation of Genesis. The dogmatic assertions of mechanistic biologists, put forward as if they had the authority of true science behind them, must I think, be extremely damaging to civilisation. The author of those manuals for the millions writes :

> ' To interpret the phenomena of living matter in the most economical terms we are driven to ask ourselves whether the properties of living matter are not the manifestations of the same general principles which *have been established* in the realm of inanimate matter ' [italics mine]. This, he says, ' is the ultimate goal of all biological enquiry.' [1]

He, and those like him, would regard Biology simply as a field for the extension of physics and chemistry. I do not doubt that all science is one, but I do question that physics and chemistry as we know them, will be found to explain the whole of science. To express what I feel I cannot do better than quote the concluding words of the Presidential Address which Professor James Gray gave to this section in 1933 :

> ' Experimental zoology can be divided into two types of study : (1) the investigation of the physical and chemical properties of living organisms ; (2) a study of the intrinsic potentialities of living matter, revealing as it does a co-ordination of events which is without inanimate parallel. In the first type of work we must use each new weapon which the physicist can give us. In the second type of work, however, biology must be the mistress and not the servant of physics and chemistry—she must make her own foundations, and build on them fearlessly, prepared to change her views, if need be, but not prepared to force the wine of life into bottles which were designed for use in the simpler and less intoxicating fields of chemical science.'

Dr. E. S. Russell, a few years ago, published a little book which I believe may be a landmark : *The Directiveness of Organic Activities*. In a witty review of this book in the *Spectator* he is described as ' clattering down the path of least resistance.' But is he ? He certainly appears to be going in the opposite direction to many biologists at the present time. Is it not possible that he, and the late Professor J. S. Haldane who took a somewhat similar view, are rocks standing firm against a flood of those biologists who have sold their souls to physics and chemistry and are streaming down the valley of quicker results ? I think it may well be so. But I didn't say that he is getting anywhere—not at present : he is for the moment standing against the stream saying wait a minute, look at this and this. He presents no main theory nor does he offer any hypothesis to be tested by experiment or observation. The importance of his book, as I see it, is that it is an excellent case-book drawing attention to what he calls directive activity as a characteristic of life. He does not regard it as purposive, because the organism, even if it were conscious, could not usually be aware of the goal towards which its activity is directed. The cases which are so interesting are those in which the activity may be variable ; if the goal is not reached by one method then others may be employed, or if a combination of methods is normally used, deficiency of one may be compensated for by an increased use of another. But it seems to me a pity that he does not take into account the evolutionary implications of adaptation. There are patterns of behaviour appearing perhaps to be directed towards a certain end but which could only, or so it seems, be the result of adaptation through selection : for example the movements of one animal which resemble those of another and so complete the deception of mimicry. Such behaviour, like adaptive colouration, has only meaning if viewed from *outside* the organism and must have been produced by an outside agent, *i.e.* selection through the action of predators. How much of directive activity is simply adaptation through selection? A great deal of it must be ; but some of the cases that Russell cites in his book certainly call for more consideration before being dismissed as such. To my mind he has done a great service as a pioneer pointing out the extent of this directiveness in nature ; but when he tells us dogmatically that we must accept it as an *irreducible* characteristic of life, then surely he remains a naturalist who refuses to become also a scientist.

Let us look again at the modern view of evolution, which as far as it goes, I fully accept. Variations are selected to bring about animals better and better adapted to their way of life. Man by his selective breeding can alter the form of domestic animals to suit, within limits, his own desires ; he selects those which better suit his needs. I am now

[1] *Principles of Animal Biology*, by Launcelot T. Hogben, 1930, p. x.

going to say something which might easily be misunderstood, and, perhaps, is dangerous to say. I am saying it only for the sake of argument, and not advocating something I necessarily think desirable. What I want to say is this. No modern biologist would doubt that if we knew as much about the genetics of man as we do about the genetics of some animals, then if mankind wished to control marriages by law, he could, by permitting some and prohibiting others, gradually in the course of long periods of time, alter the human race. Modern biology points to that, not as something desirable or undesirable, but as a theoretical possibility. If that is in fact a logical deduction from the present biological position, you will see where it must lead us. Evolution would no longer be guided from outside the species by natural selection, but by a directive activity from within the organism itself. We would see an organism directing its own evolution towards a goal in the future, whatever that might be decided to be. If that would not be an example of organic *directiveness*, then I don't know the meaning of the word.

In the hypothetical case just given, although man would be carrying out conscious organic selection, he would still be subject to natural selection by external agencies : for example the elimination of certain gene combinations less resistant than others to the attacks of pathogenic organisms. There would be an interplay between the two selections : natural and directive. We have seen that in nature there is probably a similar interplay between natural selection from without and organic selection from within.

There is another matter which I feel it only right to mention if one is not to be intellectually dishonest. There has appeared over the horizon something which many of us do not like to look at. If it is pointed out to us we say : ' No, it can't be there, our doctrines say it is impossible.' I refer to telepathy—the communication of one mind with another by means other than by the ordinary senses. I believe that no one, who examines the evidence with an unbiased mind, can reject it ; particularly the evidence of the vast number of experiments of the past twenty years and of that group of them which has passed successfully the statistical tests regarded as decisive when applied in normal scientific technique.

It is perhaps unorthodox for a zoologist to introduce such a topic ; but I do so for a reason. If telepathy has been established, as I believe it has, then such a revolutionary discovery should make us keep our minds open to the possibility that there may be so much more in living things and their evolution than our science has hitherto led us to expect. Such an idea as I am about to suggest is no doubt highly improbable and would perhaps be better kept locked in a bottom drawer ; I mention it however merely as a reminder that perhaps our ideas on evolution may be altered if something akin to telepathy—unconscious no doubt—was found to be a factor in moulding the patterns of behaviour among members of a species. If there was such a non-conscious group behaviour plan, distributed between, and linking, the individuals of the race, we might find ourselves coming back to something like those ideas of subconscious racial memory of Samuel Butler, but on a group rather than an individual basis. It would remove many of the fatal difficulties of his hypothesis. Samuel Butler's ideas were of course the logical development of Lamarck's, but thought of independently. If there was such a group habit and behaviour pattern it might operate through organic selection to modify the course of evolution : working through selection acting on the gene-complex. If this flight of fancy ever proved to be a fact, it would be a wedding of the ideas of Darwin and Mendel on the one hand and of Lamarck and Samuel Butler on the other !

If I appear to be ending in fantasy or in the spirit of harlequinade, I do so only to emphasise my conviction that we fool ourselves if we imagine that our present ideas about life and evolution are more than a tiny fraction of the truth yet to be discovered in the almost endless years ahead. We are but at the threshold of our understanding of living things. It is indeed urgent that we should press on. I have tried to suggest that if resources similar to those put into economic ecology were available for evolutionary studies in the field, we now have methods capable of giving us quantitative and reliable answers to our questions. I am sure that zoology outside the laboratory will play a part in increasing our knowledge no less important than that played by zoology inside the laboratory, but I am also sure that sound progress will only be made by maintaining a balance and a close liaison between the two.

SECTION E.—GEOGRAPHY

THE PLANNING OF LAND USE

ADDRESS BY
PROFESSOR L. DUDLEY STAMP, C.B.E.
PRESIDENT OF THE SECTION

There is little need to remind this audience of the continuous evolution of geography and geographical thought which has been so marked even within the hundred years of the existence of the British Association. From the days when geography was literally a 'writing about the earth' in its many phases and when geographers or cosmographers were the counsellors of rulers and princes over an immense range of subject matter, geography parted with, and in that sense gave birth to, a number of specialisms. What was left was indeed the arid factual bones of what had once been a living whole. It fell to the pioneers of the new geography to show how those bones fitted together, to show the logical pattern of cause and effect, to demonstrate the influence of environmental factors on human life. The work of Herbertson, E. C. Semple, Mackinder and Chisholm, together with H. R. Mill and others happily still with us, laid the firm foundations of geography as it is understood to-day. Whilst it is true that the nature and content of geography are still matters debated in academic circles and there are both protagonists and antagonists of the concept conveyed by the phase 'human ecology,' there remains a general agreement on geographical methods of survey and analysis, on the importance of field investigation and the use of the map as the supreme tool of the geographer.

It is my belief that the time has been reached when these methods of survey and analysis and further of synthesis can be and should be applied and used so as to assist in the solution of the great social problems of the world to-day. As the first Professor of Social Geography in this country I am often asked what is meant by 'social geography.' I am compelled to confess that to some extent the title of the Chair I occupy is an accident. The original suggestion was for a Chair of Applied Geography, the holder to occupy himself with the application of geographical methods to the study of current problems. But the problems involved are essentially those which concern man as a social animal —in the broad sense with society—and so applied geography is thus linked with the Social Sciences. 'Social Geography' in the title of my Chair may thus be interpreted as geography applied to the study of social problems. In the inter-war years more than one President of the British Association felt compelled to deal with the impact of science on society and the Division for Social and International Relations of Science, which continued to function during the war years, was concerned essentially with this outstandingly important field.

I feel, accordingly, that it is not inappropriate that I should attempt to deal to-day with some aspects of the Planning of Land Use. I shall be concerned in the main with some of the basic scientific investigations which I believe should precede any attempt at positive planning and in this sense I accept the dictum of my old friend Sir Leslie Scott (Lord Justice Scott) that geography is the science of which land planning is the art.

Some Basic Assumptions.

It has been said that any country has but two ultimate assets—its land and its people. The use made of the former depends upon the intelligence, the skill and the objectives of the latter. Obviously 'land' in this sense includes the resources which lie below the surface as well as those of the surface and of the associated bodies of water. The planning of land use is thus, in fact, planning for the full development and use of natural resources. Only slowly are we coming to the realisation that the resources of the world are finite and that the continuance of the human race in the long run must depend upon the intelligent planned use of those resources—especially of food producing land. This is essentially a long-term view and it is not surprising to find frequently a sharp conflict with a short-term economic viewpoint. There is also a conflict at times with the professional planner, especially the urban town planner, whose basic training is as an architect or surveyor, and who through failure to appreciate such natural factors as the disposition of minerals, or the qualities of soil and land, may

achieve a particular aim in planning only at the expense of sterilising resources in the long run vital to the human race.

The planning of land use in any country must be viewed against a world background —a world population of 2,200 millions, increasing at a rate of 20 millions a year ; more rapidly than the application of science to production can augment food supply and at a time when there are no more virgin lands available for easy conquest, only tropical lands where reclamation must inevitably be slow and costly even if eventually successful. The word planning has to some extent incurred popular disfavour. Its association with the advocacy of some particular recipe for erasing ills, its supposed dependence on theory and divorce from reality, still more the concept of a plan as a straight-jacket regulating our present and our future, have all contributed to this unfortunate position. In reality the world as a whole (and each country individually) is faced with the urgent need of developing and using its natural resources more scientifically and more intelligently than in the past. This is planning : the long-term plan is simply the formulation of certain principles and the short-term plan a specific development within the general.

Land use planning is not an end in itself ; its purpose is to secure what is loosely called an improved standard of living for mankind. I have discussed on more than one previous occasion what are the basic needs of man which must be satisfied by a proper use of land and have suggested that there are at least six. One is work, which brings to the forefront the location of industry and the fact that the majority of industries are tied by natural factors such as the occurrence of coal, or other minerals, the need for a waterside location, and so on. A second is a home and hence the whole need of land for housing and town planning in the restricted sense. A third is food—even more basic for human survival though apt to be forgotten by a nation which thinks only of food coming out of shops—and with food raw materials of animal and vegetable origin. With man's need of food we naturally link need of water. A fourth need of man is recreation for mind and body, which again has its land use aspects. A fifth need is mobility—means of communication—and so land for railways, roads and airfields. A sixth need is security and brings into the picture the requirements of the fighting services for training grounds.

Modern land planning involves the balancing of each of these claims on its quota of a strictly limited area of land. We may urge that for any given tract of land there is an optimum use in the national interest, though use at any given time is not necessarily permanent and many types of land can serve more than one use at the same time (multiple use of hill grazing for sheep, water supply and recreation is an example).

The problem is obviously acute in such a country as Britain but none the less real all over the world. Conservation of natural resources by individual countries cannot in the long run be inconsistent with world progress.

Although these principles sound extremely simple and obvious, the problem of their application is, in fact, very difficult. Determination of optimum use involves the weighing up of many often conflicting claims and there is very real danger that the conscious planning of land use, to which we are definitely committed in this country, may completely fail in its objective of improving our standard of living. Despite the existence of a Ministry of Town and Country Planning charged with the task of co-ordinating competing demands of land, our planners tend each to urge his own particular aim. Housing schemes conflict with food production ; security needs conflict with amenity interests and much that we are doing may well have to be undone if chaos is not to result.

Rural Population Densities and Rural Planning.[1]

In a country such as Britain we stand in the supremely dangerous position that long-range plans may be formulated and our destiny marked out for many years to come without an adequate foundation of factual knowledge and scientific interpretation. This is well illustrated by the present chaotic state of ' rural planning.'

It would seem obvious that, if every acre of good agricultural land is important in food production, new building in the countryside —in villages and the open country—should be restricted to building for agricultural purposes, including of course homes for agricultural workers. To avoid ' pepperpot ' and ' ribbon ' development, and at the same time to facilitate provision of essential services such as electricity, piped water and main drainage, new building should obviously be in existing villages. Let us see where these two, apparently obvious, principles may lead us.

The population of rural areas falls into three main groups :—

(a) The *Primary Rural Population* of farmers, farm-workers and their families with, in appropriate areas, forest workers—

[1] This section is based on a paper prepared for a Government departmental conference in July 1947 and not previously published though some of the conclusions were noted in *The Land of Britain : Its Use and Misuse* (p. 448) and were used by Mr. Geoffrey Clark in his paper to the Cambridge Conference of Town Planners in 1948.

the people who depend primarily and directly on the land for a livelihood.

(b) The *Secondary Rural Population*, which exists to serve the primary population and so to complete the essential make-up of the rural community. To be included are resident landlords, village shopkeepers, smiths, wheelwrights, workers in garages and servicing stations, certain of the middlemen serving rural needs, contractors for agricultural operations, together with the parson, publican, doctor, vet, schoolteachers and postmen essential to village life.

(c) The *Adventitious Population*, people who live in the country by choice. Included are many who have retired from active life from the country itself (retired farmers), from the towns or from abroad, but also many who, whilst working in nearby towns, prefer to live, and especially for their families to live, in rural surroundings.

Since the Primary and Secondary Rural Population depend essentially on the capacity of the land to support them, the first question to be answered is how many farmers can the land support? Figures for farmers in Census and Agricultural Returns must be used with caution because of the inclusion, especially in the latter, of many small holdings worked as a hobby, or part-time occupation, or to supplement a pension, by those who are not primarily farmers. For the first time, the 1941 Farm Survey gave basic data for England and Wales on *full-time farmers*. They numbered 215,900 and they were cultivating 21,296,000 acres of crops and grass or 88 per cent. of the total cultivated area. Incidentally this number of full-time farmers coincides very closely with the total number of holdings of over 20 acres. We get the important result that the average holding of the full-time farmer is 98·6 acres of crops and grass. The Farm Survey considered 10 acres of rough grazing as equivalent to one acre of improved grass so that if the 5,600,000 acres of rough grazing were included we find that an 'average' full-time farm in England and Wales is just over 100 adjusted acres. Thus over a typical stretch of, say, the Midlands of England where nearly all the land is cultivated there would be an average of rather over six farms per square mile worked as full-time holdings. Despite the many other changes which have taken place in British farming there is abundant evidence to show that this position has not materially changed over the past 80 years. The number of holdings over 20 acres has remained almost constant (the drop from 212,041 in 1885 to 206,373 in 1945 is easily explained by diminution in total farmed area) and, contrary to popular belief, there has been no tendency to consolidate holdings into large units of over 300 acres (instead the number has dropped from 16,608 in 1885 to 12,360 in 1945). The 'family farm' of 50 to 300 acres has become more firmly established than ever as the predominant unit.

The density of *farmers* per square mile has thus remained virtually unchanged but their families are smaller than they were. Though the difference in the country is not as great as in the towns in 1871 the total rural population was 3·76 times the number of males over 21 compared with 3·15 in 1931. Using these multipliers we get a density of about 24 per square mile of farmers and their families in 1871 compared with about 20 at the present day.

With farm-workers the position is completely different. In 1871 there were 675,768 farm-workers in England and Wales over 20 years of age. This is an average of rather over 3 per full-time farmer or one worker per 35 acres of crops and grass, or about 18 per square mile. Using the multiplier of 3·76 we get 68 per square mile of farm-workers and their families. We have corresponding figures from the Census of 1931 of 383,359 farm-workers or 1·73 per farmer or one per 60 acres of crops and grass or 10 per square mile. This gives a density of about 32 per square mile of farm-workers and their families in 1931. Whilst exactly comparable figures are not available for later years, the numbers of adult male workers dropped sharply from 1931 to 1939 and we may be sure that the present density of workers and their families does not exceed 30.

Summarising we find the *Primary Rural Population was*:

92 per square mile of cultivated land (crops and grass) in 1871.
50 per square mile of cultivated land (crops and grass) in 1941.

It is more difficult to calculate the *Secondary Rural Population*. A house to house survey of a number of rural parishes in Dorset carried out by Mr. Geoffrey Clark shows a Secondary Rural Population rather less than the Primary. This accords closely with the results obtained by Professor Alexander Stevens[1] who, using an entirely different method of calculation finds the total 'landward population' (i.e. primary plus secondary rural) rather less than double the primary. For two purely rural counties, Huntingdonshire and Brecknockshire, he finds the multiplier is

[1] The Distribution of Rural Population in Great Britain, *Trans. and Papers of the Institute of British Geographers*, 11, 1946, 21–53. See note below, p. 233.

1·77 and in his words, ' it seems a fair conclusion that if the value (of the multiplier) differs much from 2 for any community some special reason must be sought, and will be found, to account for the fact.'

Using these figures we thus get a normal rural population density not exceeding 100 per square mile of cultivated land in 1941 against one of 180 in 1871. This, it must be noted, is with the *same* number of farms and farmers and a considerably greater output so that ' rural depopulation ' is linked with increasing agricultural efficiency.

It is instructive to see the relationship of these figures to the population of a typical English parish. By ' typical ' we are thinking still of the well cultivated lowlands where the land is good but not superlative (2 plus 4 of the Land Classification 1:625,000 map). With village centres two miles apart, a large number of parishes approximate to 4 square miles or 2,560 acres. We see that the normal rural population at the present day is not likely to exceed 400 [1] against 720 in 1871. Now this has a very important bearing on the provision of social services but notably on educational facilities. I am indebted to my colleague Professor D. V. Glass for providing me with the following figures of school population in rural districts of England and Wales. In 1871 23·8 per cent. of the rural population fell into the age groups 5–14, i.e. of school age, or about 170 children in the parish of 720 souls and of these about 80 were in the age group 5–9 for the village school. In 1931 16·9 per cent. fell into age groups 5–14, or about 68 in the same parish and of these only 34 were of age 5–9 for the village school. If it is the policy of the Ministry of Education to eliminate single-teacher schools and to question the efficiency of even two-teacher schools we seem to be approaching the day when there will be no rural school in the average village. But the same difficulties arise in the provision of other services.

In this address I am concerned to show deficiencies in our basic knowledge rather than to suggest the answers to such problems as the one just exposed but one cannot help being forced to the conclusion that to maintain the whole social structure of rural Britain we must look to the ' adventitious ' population—that we should encourage those who wish to live in the country, whether they are actively engaged in work elsewhere or are retired. Their homes need not break up good farms, they must not detract from maximum food production but to use the Development Charge or Planning consent to prevent their entry may be disastrous. A solution which has been proposed to some of the difficulties is to select ' king villages ' or ' key ' villages for development. Whilst this has some obvious points in its favour, it would be disastrous if it led to the decay of the non-selected villages and might seriously hamper agriculture if workers or their wives and families — quite legitimately — preferred to live in the ' king villages ' far removed from their work.

[1] Incidentally these figures show that it is nonsense to talk about 600 or 1,000 population being a minimum for a ' viable ' village.

The Lack of Knowledge.

Any such survey as that just outlined inevitably brings out a lack of knowledge of fundamental importance. It also brings out the failure of the specialist to appreciate the problems in another, though closely related, sphere. It often brings out too the failure either to apply knowledge already existing or to use the results already obtained by specialist investigators in another sphere. Particularly serious is the gulf between the natural scientist and the professional town and country planner. It is perhaps especially for the geographer, trained to appreciate, to correlate and to synthesise the results obtained in many realms of knowledge, to bridge this gap.

It may be worth while to indicate some of the more serious gaps in knowledge.

A working knowledge of elementary geology always seems to me an essential part of the mental equipment of any educated man, yet the ignorance not only of planners but often, I find, of other scientists is staggeringly abysmal. Failure to appreciate the disposition of economic minerals and the behaviour of underground water are two aspects of this ignorance which must affect the work of the planner. On the other hand what really matters in a broad study of land use, possibilities of development and improvement of land is the surface geology. Yet over large areas in Britain the drift deposits remain unmapped by amateurs, academic geologists or the Geological Survey. At a time when the working of gravels affects planning as well as agriculture in every county the lack of drift maps in so many areas is indeed a tragedy. Unfortunately we must accuse many geologists of a lack of interest in the economic aspects of their findings. The lithological characters of a gravel are what matter, together with thickness, variability, character of base deposit, and so on, in exploitation.[2] Similarly the lithological characters and chemical composition of the deposits lumped as ' boulder clay receive scant attenuon from the geologist.

We must pass over the lack of soil maps and of a comprehensive soil survey—a gap in

[2] Minerals and Planning. S. W. Wooldridge and S. H. Beaver. *Geog. Journal*, 1949, *passim.*

factual knowledge so obvious as to speak for itself—whilst expressing an appreciation of the valiant efforts of the small band of workers in this field. Even here there is some lack of co-operation. The modern emphasis on the profile has tended to make some soil work less valuable to the agriculturalist, who is concerned so largely with texture, than the older work such as that of our President, Sir John Russell, on the soils of Kent, Surrey and Sussex. Some soil scientists have contended that their work is purely objective and that the determination of a soil series does not give any indication of land 'quality.' On the other hand other soil scientists claim that their survey gives the whole answer to the problem of land classification.

A great need in this country is a comprehensive survey of water resources. For many years Captain W. N. McClean urged in vain before this Association the need for his pioneer work in river gauging to be continued and extended and the present position in Britain remains chaotic—the criss-crossing of long distance supply lines; the ever-present threat to fertile farm lands suggested for reservoirs, whether by London or Leicester; and elsewhere, in strange contrast, the continued struggle to send water even more easily and rapidly to the sea.

In meteorology and climatology the gaps in knowledge are especially in the realm of microclimatology. We know, empirically, that there are certain areas, often small, which are especially favoured by some such factor as free drainage of cold air in spring or a certain combination of slope, aspect, soil type and insolation and ought in any planning of land use to be 'reserved' for agriculture, but there have been few exact surveys to help the planner. Similarly the fact that the instruments in a Stevenson screen (over grass) are at such a height from the ground as to be *above* the level of growing crops and *below* the level of ripening tree-fruits leaves an enormous field of detailed observation still needed by the agriculturalist and so in the planning of land use.

Those concerned with the planning of land use are often puzzled by the attitude of the ecologist to a primary vegetation survey. The natural or semi-natural vegetation of those great problem lands of the country, the moorland, heathlands and rough pastures, is said to indicate faithfully the sum total effect of environmental factors as stabilised by human activities such as controlled grazing, yet no detailed maps are available in most areas.

I do not regard these indications of gaps in basic knowledge as more than samples: they are not given in any critical spirit of the work of the specialists in the field concerned but rather to indicate how, at present, the work of land use planning must rest on a scientifically unsatisfactory basis.

The Survey of Existing Land Use.

It always seems to me obvious that the existing position must be the starting-point for any future development. We must start from where we are. This was the basis of the work of the Land Utilisation Survey of Britain carried out in the years 1930–39 (mainly 1931–32) which aimed to record the then existing use of every acre of England, Wales and Scotland (and has since been extended to Northern Ireland). Land use in an old-settled country such as Britain is the result of many centuries of 'trial and error' and of the interaction of many factors—natural or geographical, historical and economic. Existing land use has therefore a far deeper significance and is worthy of correspondingly more intensive study than in one of the newer countries of the world. Some of my scientific colleagues have urged that, before surveys of land use or any attempts at land classification are made, the *object* of the work should be known. I disagree. A survey of land use should be strictly objective and the results should be studied objectively. In the Land Utilisation Survey of Britain this has been done at length in the County Reports [1] and in my own summary [2] but three points may be emphasised.

In the first place such a survey is a snapshot picture in a long process of evolution and it is important to trace the history of land use. Cartographic comparison brings out the weakness of statistical comparison: cartography tended to emphasise the remarkable stability over long periods of years of the land use pattern, contrary to the large changes suggested by statistics. Remarkably little change on the poorest lands and the best lands, the maximum of change on land of intermediate quality is the answer to the apparent contradiction.

In the second place we may stress the importance of correlation—with solid and drift geology, relief, soils, and climate—which brings out the *increasing* influence of the geographical factors. This is a point I first emphasised at the Nottingham meeting of this Association as being contrary to accepted ideas. In general, with increase of facilities for transport and the development of world trade, geographical conditions must be favourable to secure economic development of land use—as in the cultivation of some specific crop against competition from elsewhere.

[1] *The Land of Britain:* The Report of the Land Utilisation Survey. 9 vols. or 92 parts.

[2] *The Land of Britain: Its Use and Misuse.* Longmans, 1948.

In the third place the separation of the influence of the various geographical, historical and economic factors and the recognition of their continuing influence suggest trends in development. Land use planning becomes, in effect, the encouragement of trends deemed desirable and the conscious attempt to reverse those considered undesirable.

The Classification of Land.

In the preceding sections we have talked about an ' average ' farm on ' average ' land. Actually few if any countries of comparable size can boast such a wide range as can Britain in scenery or type and quality of land. We may claim that some of our brick-earth soils are unsurpassed in natural qualities by any in the world and this combined with a climate which for certain purposes is also unrivalled gives a level of production of a very high order. Such soils are strictly limited in extent and correspondingly precious. Unfortunately some professional town planners have such a pathetically touching belief in the powers of science and scientists that they believe almost any land can be treated and upgraded to this level and that they are free therefore to ignore quality of land. When the Government declared its intention of conserving good agricultural land it became suddenly necessary not only to define different types of land but also to map them. The simple classification into ten types which was developed has been the subject of much discussion which I have published elsewhere in full.[1] Although Types 1 to 4 inclusive are the ' good ' agricultural lands, it must be emphasised that the ten are *types* and that it is not a question of No. 1 the best and No. 10 the poorest. For certain purposes No. 1 is first class, for other purposes No. 3. For the sake of completeness I repeat here (Table 1) a list of the types and the area each covers in England, Wales and Scotland.

This scheme of land classification has more than once been misrepresented. It is based essentially on the *unalterable* factors of elevation, slope, aspect, subsoil and drainage of the land and on the depth, profile and texture of the soil. There are some town planners who will not recognise the permanence of these factors and talk loosely about improving or upgrading land as if large areas could be transferred from one category to another. By lowering the watertable land can be transformed from Type 3 to Type 1 ; light soils may be marled and heavy soils lightened by extensive dressing of sand, lime, etc. ; but broadly speaking any upgrading is an improvement of fertility status or productive capacity within the type.

[1] *The Land of Britain : Its Use and Misuse.* Chapter XVII.

Although in such an old-settled land as Britain there is an obvious correlation between land-type and land-use it must be emphasised that the classification is not one based on *actual* land use but is related to potential use.

Apart from this correlation with land use some interesting comparisons can be made with other types of distribution. The Land Classification map obviously reflects closely the major relief of the land and the solid geology. Other features are associated with such adverse hydrological conditions as bad drainage on peneplaned surfaces and excessive rainfall. But there is also a close tie-up with Types of Farming as shown on the map prepared by the economists of the Ministry of Agriculture.

Many data of immense interest were collected by the 1941 Farm Survey. The results have been analysed statistically but only to a limited extent geographically. As part of the work of the Survey, the bounds of every farm were recorded on 25-inch, 12½-inch, or 6-inch maps. To transfer these boundaries to smaller scale maps is a colossal task, but where it has been done [2] the results are interesting in the extreme. Around nearly every village one finds small holdings and little patches of accommodation land but in the open country the farms worked by full-time farmers reflect almost everywhere the quality of the land. The poorer the land, the larger the holding needed for the family farmer to make a living. On the finest market gardening soils of Evesham 10 or even 8 acres are enough for a full-time holding and where the holdings are large on such land they are either worked far below the level of their capacity or adequately with the aid of a large labour force. At the other extreme in the Welsh uplands the place of the standard 100-acre farm is taken by one of 10 acres of crops and grass with 900 acres of rough grazing. This ' carrying capacity ' of land is also linked up with the size of parishes. Obviously there are many exceptions but in general the poorer the land the larger the parish, so that the parish populations in the very large scattered communities often approximate to figures already given. In such cases distance between settlements is an added difficulty in rural planning.

Data have been accumulated but not yet worked out showing a linkage between farm rentals and land quality. But, as I have indicated elsewhere, the correlation is not a simple or direct one.[3]

The Land Classification map of Britain obviously gives but a crude and highly

[2] By the Agricultural Land Service of the Ministry of Agriculture—at present confidential.

[3] *The Land of Britain : Its Use and Misuse*, p. 381 *et seq.*

TABLE 1

	England and Wales		*Scotland*		*Great Britain*	
	Acres	%	Acres	%	Acres	%
Major Category I—Good	*17,845,900*	*47·9*	*3,963,300*	*20·8*	*21,809,200*	*38·7*
1 First Class	1,963,100	5·3	396,800	2·1	2,359,900	4·2
2 Good General Purpose Farmland :						
2 (A) Suitable for ploughing	7,065,600	18·9	1,735,900	9·1	8,801,500	15·6
2 (AG) Crops or Grass	2,636,900	7·1	192,900	1·0	2,829,800	5·0
3 First Class, restricted use unsuitable for ploughing	1,234,800	3·3	8,700	0·0	1,243,500	2·2
4 Good but heavy land	4,945,500	13·3	1,629,000	8·6	6,574,500	11·7
Major Category II—Medium	*11,933,800*	*32·0*	*2,877,400*	*15·1*	*14,811,200*	*26·3*
5 Medium Light Land :						
5 (A) Suitable for ploughing	2,402,100	6·4	77,400	0·4	2,479,500	4·4
5 (G) Unsuitable for ploughing	220,300	0·6	300	0·0	220,600	0·4
6 Medium General Purpose Farmland	9,311,400	25·0	2,799,700	14·7	12,111,100	21·5
Major Category III—Poor	*6,350,900*	*17·0*	*12,113,800*	*63·5*	*18,464,700*	*32·8*
7 Poor Heavy Land	825,900	2·2	54,100	0·3	880,000	1·6
8 Poor Mountain and Moorland	4,516,800	12·1	12,001,700	62·9	16,518,500	29·3
9 Poor Light Land	811,800	2·2	57,900	0·3	869,700	1·5
10 Poorest Land	196,400	0·5	100	0·0	196,500	0·4
Residue—Closely Built Over	1,142,700	3·1	114,200	0·6	1,256,900	2·2
Total	37,273,300		19,068,700		56,342,000	

(The figures for England and Wales include the Isle of Man.)

generalised picture. But it *does* serve to indicate the small extent of the good lands and points to the tracts which should be avoided in the siting of new towns if the country's productive capacity is not to be impaired. In more detail the application and refinement of this method is being used to indicate the directions in which a given town may expand with least damage to productive capacity.

It will be noted that I use the words ' productive capacity ' and not production. So many town planners insist on using only existing production from what may be a badly farmed area to minimise their estimate of loss incurred by the schemes of urban expansion they may be advocating. Thus 50 acres of well-managed land of Type 3 will support a dairy herd of fifteen 1,000-gallon cows, giving a yield of 30,000 gallons a year per 100 acres. This is not the actual yield of the 1,243,500 acres of Type 3 land shown on the map, but if we are minded to indulge in calculation and speculation we can gain a glimpse of the country's productive capacity.

Two Settlement Patterns.

Britain has two distinct settlement patterns superimposed on one another. The firmly entrenched rural social structure of landlord, tenant-farmer, and farm-worker persists despite the increase in numbers of owner-occupiers and the waning fortunes of the harassed landowner. It has resulted in the extremely widespread pattern of ' average ' farms with farmsteads conveniently placed for each holding and so scattered. Some of the farms have ' tied ' cottages for workers, but the remaining population of the parish is grouped in one nucleus (nucleated village) or several (hamlets). The squire's house (if with a large park usually on the poorest land), the church, rectory, inn and stores form the basis of the village together with the cottages of the agricultural workers and secondary rural population. Where water supply is a problem or defence formerly was, the farmsteads themselves may be in the village. A group of villages needed a market hence the market towns originally so spaced as to be within reach on foot for man and beast. The development of roads and improved transport at once explain the growth of the better placed market towns and the decay of the remainder. Our mediaeval ancestors were wise : they knew their sustenance came from the soil and they did not cover valuable land with buildings. Thus the early manufacturing towns, supplying the needs of the countryside, often grew up as Birmingham did on tracts of unproductive land.

Superimposed on this still existing rural pattern is the pattern of modern industrial-commercial Britain. In the great growth following the Industrial Revolution the urban spread was mainly on the coalfields and for the greater part on poor land—Sheffield, Leeds—Bradford, Manchester with Ashton, Bolton, Bury, Rochdale, the Black Country, the Welsh valleys are all good examples. Though the working of the deeper parts of the coalfields affected good land it is the partial emancipation of industry from the coalfields by use of electricity which has provided a major threat to the continued existence of the best food producing lands. This threat is reinforced by the timidity of town planners or parsimony of central and local authorities in seeking level or near level sites. It seems to be accepted far too widely that a satisfactory new town or housing estate depends primarily on more space, ignoring the fact that the majority of our beautiful towns and cities owe much to an intelligent use of varied, often difficult, natural sites.

The two settlement patterns are superimposed rather than fused and the question arises : how far are their established features permanent ?

It has already been pointed out there has been remarkably little change over the past 80 years in numbers of farms and consequently of their size. Are changes likely in the foreseeable future ? Since it seems that the world trend in farming is towards the British model of the mixed family farm, cultivation-machinery is more likely to be adapted to British needs than British farms reorganised to use large-scale American machinery designed for the already obsolescent prairie type of monoculture. If adjoining British farms are more economically worked as larger units, redundant farmhouses will become country residences or homes for foremen. But any such tendency to amalgamation is offset by the deliberate creation of statutory smallholdings on which selected nominees can learn the business of farming. The suggestion therefore is that the overall rural pattern will remain.

Except for certain areas of land reclamation such as around the Wash or areas marked out for specialised agricultural development such as Romney Marsh, it should be noted that the rural pattern is complete over the country. Farms may be realigned, villages strengthened by additional cottages, market towns revivified by industry but there is no room for new villages or new towns as part of the rural pattern.

Not so the urban pattern. Seemingly oblivious to the fact that the land area is fixed and inextensible at 37 million acres for England and Wales and 19 million for Scotland —if we ignore small extensions possible by

TABLE 2

Possible Changes in Land Use in Great Britain

Pre-war Land Use					*New*		*New*	
Acreage	*Use*	*%*		*Possible Change*	*%*		*Acreage*	
1,000,000	Arable—intensive (market gardening)	2·0	→ No change		2·0	5·0	2,820,000	Arable—intensive
			→ Arable—intensive		3·0			
11,069,400	Arable—farm crops	20·0	→ Arable—farm crops		14·5	27·5	15,500,000	Arable—farm crops
			→ Orchards and fruit		1·0			
261,300	Orchards and fruit	0·5	→ No change		0·5	1·5	845,000	Orchards and fruit
18,897,500	Permanent Grassland	33·0	→ Arable—farm crops		10·0			
			→ Grassland		22·0			
			→ Arable—farm crops		3·0	25·0	14,100,000	Grassland
			→ Grassland		3·0			
18,775,200	Heathland, Moorland and Rough Grazing	33·0	→ No change—rough grazing, sporting, national parks		15·0	15·0	8,450,000	Heathland, Moorland, Rough grazing
			→ Forest and Woodland		10·0			
3,219,200	Forest and Woodland	6·0						
	High Forest (3·0)		→ No change		4·0	16·0	9,000,000	Forest and Woodland
	Coppice (1·0)		→ Coppice		1·0			
	Scrub & uneconomic (2·0)		→ Uneconomic		1·0			
1,719,900	Houses with gardens including allotments	3·0			6·0	6·0	3,380,000	Houses with gardens
1,399,200	Land agriculturally unproductive, including airfields, training grounds, etc.	2·5			4·0	4·0	2,246,700	
56,341,700		100·0			100·0	100·0	56,341,700	

The acreages on the left are those calculated from the Land Utilisation maps, and include ponds and other small areas of inland water.
'Great Britain' includes the Isle of Man.

expensive reclamation of tidal flats—demands upon land become ever more extravagant. In place of the Victorian industrial towns with 64 houses to the acre town planners now demand an acre for 8 or 10 houses. The standards for the new towns, taken over all, are one acre per ten *persons* so that 6,000 acres are demanded for a town planned for 60,000. In 1925 one-tenth of the surface of England and Wales, according to the Scott Report, was 'developed' for towns, villages, roads, etc. If the present trend continues the *Times* has calculated 15 per cent. will be so used by 1962 or 1967. Since towns cannot be built on mountain tops it becomes more realist if we say this is nearly a quarter of all the farmland of the country. In the words of Lord Radnor, we seem to be heading for 'new towns to starve in,' for we must remember that every 1,000 acres of average land taken robs 1,790 people of their share in the food-producing land of the home country.

If we turn back to the table of land classification we see that land of medium and poor quality covers more than half the country and that, avoiding the obviously unsuitable areas, there is room both for the proper rehousing of our people and the maintenance of home food production—or at least the conservation of the land for that purpose. I see no reason to revise seriously a calculation I made some years ago of actual and potential use of the land of Britain (Table 2).

But this is a glimpse of speculation into the future, and my main aim in this address has been to appeal for a detached scientific approach to the work of the planning of land use.

NOTE.—Recent work by Mr. S. W. E. Vince in East Anglia suggests that the Secondary Rural Population which exists to serve the Primary alone is only about half the latter so that the multiplier (above p. 226) in purely rural districts is about 1·5. He finds a remarkably close correlation between the carrying capacity of the land in terms of primary rural population and the classification of land outlined above.

SECTION F.—ECONOMICS

ECONOMICS: YESTERDAY AND TO-MORROW

ADDRESS BY

SIR ALEXANDER GRAY, C.B.E.

PRESIDENT OF THE SECTION

THE natural hesitation which I feel in addressing you as President of this section is perhaps slightly alleviated by the reflection that I am now (I believe) entitled to speak to you—if I may lapse into French—as the doyen among the Professors of Economics in these islands. It is an honourable position which I owe solely to the lapse of time,—a process which ultimately brings distinction to the meanest centenarian, as a reward for the somewhat negative virtue of not having dropped out by the way. It is rather like Interest (or at least what Interest used to be before 1935), the Reward of Waiting, attainable even by the mentally deficient, who may doubtless qualify by virtue of 'long lacking.' It is not far short of fifty years since I was first introduced to the study of Economics on a mixed diet of the small Marshall and John Stuart Mill; I have now been professing since 1921. In this section we are not all 'Academics' (a horrid word); but perhaps we Academics give a flavour to the proceedings. In the circumstances I think it may not be inappropriate if to-day I speak to you as a Victorian Professor on his last legs. You may consider that in the string of platitudes which I shall doubtless address to you, I am confusedly making a death-bed confession of error; or (if you are more charitable) you may regard me as doing my best to understand sympathetically how, in the very different climate in which he will carry on his activities, my more enlightened successor may regard me.

We could doubtless learn a great deal of the continually changing view of what Economics is supposed to be, and what purpose its study is supposed to serve, by the simple device of reading in chronological order the definitions of Economics given by its leading exponents. But this is a task which to-day I may not perform on your behalf. A random stab here and there must suffice. Adam Smith is entirely explicit in one passage frequently quoted in better days: 'Political Economy, *considered as a branch of the science of a statesman or legislator*, proposes two distinct

objects.' These objects do not concern us at present ; but you will remember that they were, firstly, to provide a plentiful subsistence for the people, or, as he at once added (suddenly remembering how omnipresent is the danger of misrepresentation), ' to enable them to provide such a subsistence for themselves ' ; and, secondly, to provide a sufficient revenue for the State. But the essential point to note is that for Adam Smith, Political Economy was a branch of the science of a statesman ; in other words it was inextricably intertwined with politics and government, concerned with getting things done. I do not think that the idea of there being such a thing as an ' economic law ' ever penetrated the crass intelligence of Adam Smith ; I doubt whether the word ' Law ' in this sense occurs anywhere in *The Wealth of Nations* ; but when you come to Ricardo, it is hardly too much to say that the primary function of the Economist is to detect and lay bare ' Economic Law.' Indeed the characteristic feature of the early part of the nineteenth century was the conscious effort to transform Economics into a wholly independent scientific discipline, establishing laws or generalisations, while remaining perpetually and eternally neutral in a world of conflict. You will find a sufficiently beautiful expression of this simple view in Senior, who chides previous writers for their incorrigible tendency to wander into what he calls the far more interesting, but far less definite, fields by which the comparatively narrow path of Political Economy is surrounded. It is an arresting simile. The economist is a dumb ox, condemned to follow a narrow path which leads through attractively flowery, but not very definite, meadows; but woe to him, if he looks over the hedges. ' His conclusions, whatever be their generality and their truth '—it is Senior who is speaking—' do not authorise him in adding a single syllable of advice.' Much later in the century you will find the same point of view expressed by Cairnes, with comparisons which I find not too happy : Political Economy (he said) stands ' neutral between competing social schemes ; neutral, as the science of Mechanics stands neutral between competing plans of railway construction . . . ; neutral, as Chemistry,'—and this is what sticks in my throat—' stands neutral between competing plans of sanitary improvement.'

Indeed in an era of *laissez faire*, the divorce between Economics and Politics is perhaps inevitable. Let us for the moment accept the familiar travesty of *laissez faire*, and assume that it postulates that the ideal State should confine itself to doing nothing, and doing it rather well ; but if that is even an approximation to the implications of *laissez faire*, then it is difficult to see how Economics, however defined, can be subservient to Politics. For though there may be a certain Art in doing nothing, it may be questioned whether there is a corresponding Science.

Doubtless it was always realised that Economics had its neighbours, that there were frontiers and borderlands ; and those of us who were properly brought up and learned our ' Scope and Method ' from Dr. Neville Keynes, at one time in our lives knew all about the ties between Economics and the neighbouring tracts of country occupied by Political Science, Ethics, Psychology, Jurisprudence and many more. Doubtless also the most resolutely recluse economist perpetually violated his own professed principles ; for it is as difficult to prevent an economist from giving advice as it is to prevent the notional proverbial woman from talking. Yet, broadly, despite obvious lapses, it is true that the nineteenth century was in the main a period of specialisation, when the economist tried to keep himself to himself, and to pursue the peculiar problems of his own specialism.

It is almost a commonplace to-day that the era of specialistion has for the time-being ended ; that the old frontiers have been largely obliterated, and that in the next generation the economist will have to be much more than an economist. Perhaps his primary task will be to view his Economics in relation to all the other so-called social sciences, and thereby to effect a new synthesis. Let us consider for a moment, very briefly, some of these increasingly non-existent boundaries. The first and most complete disappearance of a frontier is, I think, that between Economics and Politics. For myself, I regard this as having always been a very blurred and indeterminate dividing line ; but that is merely a personal point of view. To-day it is possible to say more emphatically than at any time since the dominance of mercantilist ways of thinking, that practically every economic problem has become a political problem, and that most, if not all, political questions have their economic penumbra. The more or less complete fusion of economics and politics can be explained on various grounds, even if the various explanations very largely represent different aspects of the same fundamental tendency. You may say that it is due to the coming of the Welfare State ; you may say that it is due to the eclipse of *laissez faire*. In an age which has, apparently, unbounded faith in the competence of the State to do everything and to provide for everything, the decision in economic problems tends to pass to the government, and economic discussion finds its last asylum in the House of Commons. As an influence tending to the

fusion of Economics and Politics, you may attribute something to the delayed impact of Marx, for whom Economics was a first stepping-stone to revolution, which is political, if anything is. The extent to which Economics and Politics have to-day become intertwined is seen in the fact that what is probably the most fundamental, as it is the most unbiquitous, problem of the times is precisely that of determining the limits of the economic activity of the State—what we expect the State to be, and what we expect it to do in economic matters.

The second frontier which has obviously been trampled upon—or perhaps should be trampled upon even more than it is—is that between Economics and Psychology. We ought never to forget that we are always dealing with human beings, though perhaps we sometimes tend to do so when we wrap ourselves in a cloud of differential equations. But if we are dealing with humanity, then we ought to know everything that there is to be known about the curious behaviour of this most curious human beast. We should remember that we are never far removed from a suffering human heart. Here possibly, as I sometimes think in moments of intellectual pessimism, will for ever be the weak point in Economics, and indeed in all the social sciences. For the heart of man is unsearchable, and his behaviour can never be foretold, moved as he not infrequently is, and moved surprisingly, by motives of which he is himself unconscious. Strictly speaking there is no such thing as Labour, glorified with a capital 'L,' apart from individual labourers who all toil and sweat at different jobs, and display an infinite variety of temperament, disposition, endowment and circumstance. There is also no such thing as Capital, apart from the individual owners and controllers of the means of production; and these again, I need hardly tell you, are a motley crowd.

The older generation suffered much ridicule—undeserved, as I think—because of what was supposed to be the undue simplification of their underlying psychology. But indeed there is something to be said for them. They realised that if they were to reason about Man, they must first define him; just as, before reasoning about triangles, you must know what a triangle is. And as a result there emerged, or was supposed to emerge, that ridiculous, bloodless scare-crow, known as the 'Economic Man'—the man whose every action was determined by a careful balancing of economic considerations. Yet, if he existed anywhere, I doubt not that he was a most exemplary citizen, observing the laws of God and of Man. He remembered the Sabbath day to keep it holy, being advised by his medical attendant that one days' rest in seven would increase his productivity in the remaining six. He honoured his father and his mother, knowing well how wide is still the testamentary discretion which the law allows to parents. Of the grosser offences he was guiltless, being well aware that a period of incarceration would effectively debar him from the profitable service of his fellows. If he had any weakness, it was doubtless a tendency to covetousness, but such slight shortcoming as he may have been guilty of in this direction was, he felt, no more than was inevitable, if he were to comply with the Pauline injunction to covet earnestly the best gifts. Such, supposedly, was the Economic Man, and though he had his virtues, 'Who would not laugh if such a man there be?' Nevertheless, looked at more closely, what the older economists were trying to do was, in their own words and for their own purposes, to 'make abstraction' of those qualities which did not affect man in his acquisition or consumption of wealth. Political Economy was concerned, such was Mill's view, with Man solely as a being who desires to possess wealth, and who can make efforts to that end. Other qualities of the human beast must be ignored, except in so far as they are 'perpetually antagonising principles' to the desire for wealth. And of such perpetually antagonising principles, there are two. In a very fine phrase, they are 'aversion to labour, and the desire of the present enjoyment of costly indulgences.' Man, in short, is a lazy brute, and he wants to have a good time *now*; and if we are honest with ourselves in our private confessionals, we will acknowledge, each speaking for himself, that it is at least a part of the truth, even if we should prove incapable of clothing the confession in the impeccable language of John Stuart Mill. But it was never meant to be a complete picture: still less did that saint of Rationalism intend it to be a picture of the ideal man. Mill, in effect, said: 'I cannot tell you what Man is; but this, alas, is a bit of him.' And also implicitly the Economic Man was a warning regarding the limitations of the validity of economic conclusions. In a sense Mill also said: 'I am talking of one side of Man; but he has other sides, and therefore you had better be on your guard.' In these days when we do so much to encourage research, I should like some modern John Bunyan to supplement *The Life and Death of Mr. Badman* by a companion volume on *The Life and Death of the Economic Man.* The research student who is conspicuously distinguished by aversion to labour will find a great deal of the necessary biographical detail in Dr. Hennipman's imposing volume. For if the Economic Man is not dead, at least we seem to have buried him. Yet incomplete

and defective as this ogre may have been as an explanation of human action, he was at least an acknowledgment that you do need a psychological foundation. I am not sure that we are further on to-day. If any one were to ask me what are the psychological assumptions of modern economics, I should be constrained to give a dusty answer. Perhaps the ghost of the Economic Man, that perturbed spirit, still stalks our ramparts. But of one thing I am certain : a structure of economic theory that is not based on sound psychology is a house without foundation. And therefore your economist must be a psychologist—of a sort.

I must deal more briefly with some of the other disappearing frontiers. In my youth there was much talk of the relation of Economics to Jurisprudence. Apart from all that was then said, about it and about, I suggest that in the new world there are overwhelmingly stronger reasons why every economist must become something of a lawyer. In the Welfare State, the law provides the machinery for the economic activity of the State ; the machinery rests on and is modified by the law. To-day the economist must read and, so far as that is practicable, understand a considerable section of the enactments that are being incessantly disgorged from Westminster—even if Members of Parliament themselves are exempted from this exhausting task. It used to be said that the best source-book for a study of mercantilism could be compiled from the Preambles of Acts of Parliament, in days when Acts of Parliament and their preambles were still models of reasoning and of English style. To-day one of the chief sources for the economic tendencies of our times is in the Statute Book itself ; and an economist must be at least of the competence of a hedge-lawyer to find his way about.

Need I say anything about Ethics, our nearest and our oldest neighbour ? You will recall Marshall's saying that Economics is the handmaid of Ethics. Admittedly, handmaidens to-day are more of a notional race of beings than they were in Marshall's life-time ; but the idea is clear and its justification incontestable. Let us admit, even in this Section, that Economics can never be an end in itself ; by its very nature and from its birth it is doomed to be a suborinate, a younger sister, a handmaid to some one. At its best Economics is designed to disclose a means to the attainment of some end, to the realisation of some way of life, some vision or some ideal. It is for Ethics (we hope) to tell us what the good life is, what justice and equity are, what sort of an existence we ought to try to provide for ourselves and others—if it comes to that, how we ought to behave to each other, a question which not infrequently has curious economic infusions. But those who are concerned with the means can hardly be denied the privilege of having some views on the end. It is indeed only in so far as we take some part in the ethical debate that our Economics can aquire a meaning, an inspiration and a driving force. If you ask me to fix a just wage, you must allow me to have some ideas about ' justice.' And I think that we economists should perpetually bear in mind that the framework of *The Wealth of Nations*—which we may conveniently take as the source of modern economics—was first presented to raw Scottish undergraduates as part of a course on Moral Philosophy. We economists were conceived in the study of a Professor of Moral Philosophy, and cradled in a Moral Philosophy classroom. Whether you believe in the laws of inheritance or in the potency of early environment, you can hardly deny that it is ' natural ' for an economist to be a moralist.

And having gone so far—Heaven help me ! —may I go still further and suggest that the economist may have to become something of a theologian ? Or if for any reason you shy at some of the ordinary implications of the word ' theology,' let us say that the economist should aspire to a reasoned *Weltanschauung*—for there is always much wisdom in taking refuge in a foreign language, imperfectly understood. Doubless it is at times difficult to make sense of this world ; yet I suppose it is what we are all trying to do. And unless we are prepared to regard ourselves merely as a company of irresponsible idiots dancing in the void, we must frame for ourselves, if only as a working hypothesis, some sort of view of the kind of cosmos which we inhabit, and of the manner of its governance. The economy of heaven may be dark, as one of our poets suggested, using a very familiar word in rather an archaic sense ; but unless we can persuade ourselves that life in some way makes sense, the significance of our economics will likewise evaporate.

I have been led on to this, perhaps rather delicate, subject, because one of the most striking features in the economics of the Founders, above all in Adam Smith and Malthus, was the strong infusion of theology at the roots of their thought. *Laissez faire* nowadays is a doctrine that has, apparently, fallen so low that, with few exceptions, there be none so poor as do it reverence. In an ignorant age, which to its disgrace has allowed Adam Smith to go out of print, it is commonly assumed to be a godless, damnable doctrine of selfishness and of 'devil-take-the-hindmost.' But in its origins, as represented by Adam Smith, it is something entirely different. For him the justification of *laissez faire* is to be

found in the perfection with which the Almighty has done his job. Men are unwise and curiously planned—no doubt. But the Creator deliberately made men unwise in certain respects and planned them curiously in others, so that human weakness and folly might be powerful levers to make the great world accomplish its daily round. The folly of men indeed reveals the wisdom of God. It may not be popular doctrine in these days, but Adam Smith's answer to the race of planners would have been simple. He would have said that God had already done all the planning necessary, and that your bungling, earthly planners can, at best, merely upset the divine plan. Into these troubled waters I may not enter : I am merely concerned to remind you that if Economics grew out of Moral Philosophy, it is also true to say that in its modern origins, it relied on and drew strength from a system of Natural Theology.

Perhaps I have delayed you too long on this question of disappearing frontiers ; I admit that I am tempted to consider one more example, recalling as I do with a fascinated curiosity a second-hand book catalogue I once received in which there was a section devoted to ' Political Economy and Crime.' But I do not press the point ; though there may be something in it. Nevertheless it is time to bring these conclusions together. The nineteenth century was an age of specialisms. To-day the boundaries between the Social Sciences (if we must use the phrase) have broken down. The economic problems to be solved present themselves inextricably embedded in a whole mass of relationships which cut across purely economic considerations, and involve all phases of the manifold problem of men living in society. I do not say that we must therefore cease to be economists. But more than ever, an economist, to be an economist, must be vastly more than an economist. You may recall one of the most memorable of Lord Keynes' asides, where he emphasises that the Master Economist must possess a rare combination of gifts : ' He must be mathematician, historian, statesman, philosopher—in some degree. . . . He must study the present in the light of the past for the purposes of the future. No part of man's nature or his institutions must lie entirely outside his regard. He must be purposeful and disinterested in a single mood; as aloof and incorruptible as an artist, yet sometimes as near the earth as a politician.' Such was Keynes' vision of the Master Economist ; and to this model the rank-and-file economist should also (if it is not asking too much) endeavour to conform. He must be skilled in many fields ; if he is merely an economist, he is damned.

In a world where we have removed the customs-barriers between ourselves and our neighbours—in which the economist, invading the field of Ethics, confesses that he has nothing to declare—we shall of necessity pursue our studies in a somewhat different climate. May I approach what has always been a somewhat delicate question in the history of Economics, viz., the relation between pure theory and the direct observation and study of the raw material presented to us by this clamant world ? Just as the individual professor of Economics spends his life alternately neglecting his lectures and neglecting the world on which he lectures, periodically throwing a sop to his conscience by varying the field of his neglectfulness, so Economics itself has its periods of devotion to theory and to what, not very happily, is sometimes called ' realistic studies.' Doubtless what I am saying reflects my own limited tastes and equipment ; yet I have a feeling that recently theory has been running away with us. It is in danger of becoming a field for airing and exercising our academic ingenuity. Take this question of ' Value '—the central, indeed at times the exclusive, subject of economic speculation, so much so that at one time Economics was renamed ' Catallactics,' though I rather think the new name was washed away in the course of the baptismal ceremony. We have discussed the subject of Value for a solid 2,000 years ; but it may be doubted whether we have made much progress. Such advance as is periodically made seems indeed to consist largely in a dawning realisation of the inadequacy of the recently dominant theory. It may be that none of us knows—any more than Marx knew—what we mean by Value : and that in itself perhaps renders the formulation of a satisfactory theory of Value a matter of some difficulty. In my life-time, we have brooded on the margin, when brooding on the margin was still a fairly recent escape from the summation of past costs. More recently we have been sliding down curves of indifference, exchanging cigarettes against tots of rum, according to our varying individual tastes and preferences, or tobogganing over surfaces of indifference when we introduced a third commodity (chocolate) to render life almost unbearably complicated ; and, I presume, ploughing our way through the fourth dimension, if we were so rash as to contemplate a world where insatiable man demands four commodities. Not that I would deny or decry the place of theory, or indeed its inevitability and supreme importance. Man, among other things, is a questioning animal, and it is of the nature of the ruminator to ruminate and of the theoretician to theorise, in accordance with the will of God.

And retrospectively, if only as a key to the interpretation of history, nothing can be more important than theory, for in the last resort it is ideas that move men. Lenin rests on Marx ; and Marx at least thought that he was a theory of Value. If you care to be fanciful—and I admit that you would be fanciful—you might contend that the Iron Curtain which to-day divides Europe has as its foundation-stone (if curtains may be allowed foundation-stones) a theory of Value which the West has discarded.

Yet I cannot help feeling that in the next generation of re-orientation there may be more important tasks than the elaboration of theory far above the heads of all but the experts, in fields, moreover, in which the experts have quite obviously considerable difficulty in apprehending the drift of the arguments advanced by their fellow-experts. We are privileged (if it is a privilege) to live in an era of intense economic excitement. Never was there a time when so many experiments were being launched in the economic laboratory. Never was there a time of such far-reaching institutional change. The whole structure of society, of government, of the machinery of administration is undergoing rapid and continuous transformation. Perhaps in these circumstances our first duty is to keep our eye on the Coal Board, and on the Ministry of Town and Country Planning—and much more ; in fact, perhaps for the next ten years we shall be constrained to revert, somewhat but not overmuch, to a point of view resembling that of the Institutionalists.

In this new world where Economics and Politics are so closely entangled, and where the subject matter with which we are concerned is so often a matter of dispute in party politics, the position of the economist, if he is to retain his professional integrity, is rather a difficult one. We may not be greatly admired ; but, Heaven knows, we are made use of. The time which Professors of Political Economy can spare from the neglect of their ordinary duties is in most cases swallowed up by service on Government committees. At the same time our chief function as teachers nowadays seems to consist in training up a race, not so much of economists as of economic advisers who go forth to hold their own in Whitehall and elsewhere.

This trade or profession of Economic Adviser is one of the most interesting by-products of our new world ; and I watch all economic advisers curiously. If we are experts, we ought without doubt to be able to say something worth listening to. But let us, just for a moment, be modest, and acknowledge that we do not possess the final answers, nor are we the repositories of ultimate wisdom. There never was a more foolish question than that which I have had addressed to me : ' Why don't you economists put the world right ? ' prefaced sometimes by the reproach : ' Look what a mess you economists have made of the world ! ' One might, somewhat feebly, retort by asking why the lawyers, or the moralists, or the Church have failed to put the world right. For, so we hope, we are all employed on the same job. But probably there is a better answer than a vulgar *tu quoque*. If a combination of Aristotle, Adam Smith, Karl Marx, the Archangel Gabriel and Sir Stafford Cripps were to arise, and proclaim, with all the guile of Machiavelli, the distilled essence of economic wisdom, people would not necessarily act accordingly. The problem of government is essentially one of expediency ; above all, it is concerned with the practicable, the very urgent question of how to keep going for the next six months. There is no point in offering the country the clotted cream of economic sapience, if Parliament and the electorate will refuse the draught. There is this amount of truth in the old doctrines of Senior and Cairnes already cited. The enunciation of a flawless theory of wages (if such could be formulated) will never still the passions which have given rise to a strike. And accordingly —I am on the point of speaking blasphemy ! —the economic adviser should at times be able to forget that he is an economist, and indeed to realise that there may be occasions when he ought to forget his Economics. He is doubtless an expert witness ; but his chief value lies in the fact that he ought to be a sympathetic observer of the human scene, one whose awareness of certain aspects of life has been sharpened ; one who knows what to look for ; who knows what questions to ask.

So far I have been concerned in the main with yesterday and to-day ; what about the economic climate of to-morrow and the day after to-morrow ? We are moving, as we all know, to a world very different from nineteenth-century Victorianism. It follows that the economist's task and his problems may very well be different in the next generation. I need not tell you what are the promised characteristics of this new age, and of this new State. It is what, for convenience, is quite usefully described as the Welfare State, a State responsive to the material needs of all its subjects ; providing complete and adequate security against all the sinister contingencies of life ; abolishing want and unemployment ; giving education up to University standard (and beyond) to all who desire to avail themselves of it. In short it will be what I recently saw described somewhat derisively—I am glad it was by an American

writer—as the Santa Claus State. I have also heard it depicted, with even more derision and cattiness, as a world in which the entire population will have breakfast in bed. It will also be a world in which the State, directly or at one remove, assumes responsibility for the conduct of the major basic industries; a world also in which economically the individual will have largely disappeared. Society indeed has already undergone a process of coagulation, so that, if we count for anything at all, we count solely as a member of our appropriate group. Moreover, let us not forget that these groups may have conflicting interests.

Now in such a changed world where many things, if not all things, will have been made new, the politico-economic problem may be very different from that to which we older people have been accustomed. By the politico-economic problem I mean the problem of how to live together (always a difficult matter), and how to keep things going. It may seem a perverse thing to say; but just as life in heaven (or in some heavens) may not be altogether easy, so possibly life in the agreeable world of the future may present peculiar difficulties of its own.

There are, I think, three ways in which we may manage to live together in the complete Welfare State: or perhaps it would be better to say that there are three preliminary conditions, any one of which, if satisfied, would enable us to do the trick. The first is that of relying on a degree of compulsion vastly greater than we have yet had the courage or the honesty to admit may be necessary. I am not now pointing out the horrors of the Road to Serfdom, or of what awaits you when you come to the end of the road. In its higher altitudes this is already a well-discussed topic. I confine myself to the superficial and indeed the platitudinous. A State cannot undertake to provide from under the counter whatever any one may need unless simultaneously it sees that some one is putting under the counter what is required for the purpose. The State cannot promise every school-child a glass of milk at 11 o'clock, unless it has directly or indirectly the corresponding number of cows standing at command. May I refer you to John Ruskin, a writer whom ordinarily I would not commend for his economic insight? 'Finally I hold it for indisputable that the first duty of a State is to see that every child born therein shall be well housed, clothed, fed and educated, till it attains the years of discretion.' (So far, an excellent definition of the Welfare State, even if its concern with Welfare is limited to the years of indiscretion.) 'But,' he goes on, 'in order to the effecting this the Government must have an authority over the people of which we now do not so much as dream.' It ought to be fairly obvious that the State cannot guarantee every one against want, unless it reserves the right, if need arises, to take any one forcibly and pack him off to Caithness or Cornwall to do whatever requires being done there. And sooner or later the time may, probably will, come when it will have to realise that it must not be too mealy-mouthed or timorous about the exercise of these powers of compulsion with which, on Ruskin's view, the Welfare State must arm itself. The State cannot give what is not there. Indeed, in a sense the State, of itself, cannot guarantee anything or any standard of life. It is an old criticism of the Anarchists that the State is for ever sterile. Properly understood, it is an entirely true statement. It can act only through its subjects; and in this matter of distribution, it can only redistribute what its nationals produce or what can be got from other nations in exchange for their products. And if Plague comes, fortified by the colorado beetle, potato disease, foot and mouth disease, blockade by the enemy and all the other horrors in the Malthusian repertory, a government guarantee of a standard of life will get you nowhere. The power of the government to give is for ever limited by what the people themselves produce.

Indeed in this matter I am inclined to carry my pessimism still further. It is the tritest and most hackneyed of platitudes that rights must for ever be accompanied by duties; but though we invariably pay lip-service to the well-worn dictum, in fact our eyes in these days are morbidly fixed on our rights, whereas our duties, after a vague and perfunctory wave of the hand in their direction, are allowed to fade into the background. The Universal Declaration of Human Rights is in this respect an illuminating document. Now in the economic field a right is something that you get from some one else, whereas a duty is what we do for another. And a society in which each member concentrates on getting rather than on giving has lost the roots of its stability.

If then, in the words of the Universal Declaration of Human Rights, the Welfare State is going to provide every one 'a standard of living adequate for the health and well-being of himself and of his family, including food, clothing, housing' and much more, it must, in the last resort, have power to compel its subjects to see to it that the national bins are kept full, and that there are ample reserves under the counters of the national stores.

It may, I think, be proper to note one psychological rock which a frank policy of compulsion might encounter. It may appear

a paradoxical, and indeed a nonsensical way of putting it, but even a policy of compulsion must rest on consent. The bulk of the population must at least realise the necessity of compulsion, and be willing that others vicariously be coerced. It is because the need for compulsion is so much more obvious in war, that war-time coercion can be so surprisingly successful. But it is otherwise in peace when the need for compulsion is, for many reasons, much less obvious. We are doubtless learning in these years of austerity and adamantine budgets. Yet I think that our problems have been made harder for us by long insistence in certain quarters on the phrase which speaks of ' Poverty in the midst of Plenty '—or more discreetly, ' Poverty in the midst of the possibility of Plenty.' It is a phrase that goes as far back as John Gray. On the contrary, the wolf is ever at the door ; and throughout history a large section of the population of the world, taken as a whole, have been down at starvation level. I think that perhaps we may have bemused ourselves with speculations about the so-called Laws of Diminishing and Increasing Returns. Admittedly—*given the material out of which such things are made*—it is possible to belch out motor cars, and utility trousers and gadgets of all kinds, as never before. But let us be crude : Man lives not on motor cars and gramophone records, but by what he puts in his belly. We have doubtless all chided Senior for his somewhat too facile identification of Diminishing Returns with Agriculture and of Increasing Returns with Manufactures, pointing out (quite correctly, and with professorial complacency and superiority) that there are cases where Increasing Returns are manifested in Agriculture, just as in certain circumstances Manufactures may operate to reveal Diminishing Returns. But perhaps in the long run (most blessed of phrases) Senior may not have been so far out. The ineluctable fact is that the human race draws its sustenance from a distressingly thin layer of soil on the surface of the globe, and it is not growing any bigger Perhaps misguided by the population tendencies of Western Europe, we may have been inclined somewhat prematurely to regard Malthus as a back-number, as a ghost effectively laid by our most admirable and ever-increasing moral restraint. For the world as a whole, population is increasing, and increasing rapidly ; the supply of food is not increasing proportionately. One authority tells me that in 1960 world agricultural production may be from 20 per cent. to 25 per cent. above the level of 1930, whereas world population over the same period may show an increase of from 30 per cent. to 35 per cent. Assuredly we are as yet hardly in a position to speak over-confidently of the arrival of an era of unlimited plenty.

So much for the first method of meeting the future, the method of compulsion and direction—that degree of authority over the people of which John Ruskin alone was capable of dreaming. You do not like it ? No more do I, though I am of a more submissive disposition than most. But economically, it might be an efficient system, if the rest of the population were as submissive as I am, which probably they are not. From all we know of the Incas and the Jesuit settlements, it might be for certain placid peoples a highly efficient system indeed, producing on the material level a remarkably high degree of comfort and well-being. If compulsion is to be condemned, it must be on moral rather than on economic grounds ; it is to be abhorred, above all, because it involves a denial of personality and an abrogation of responsibility.

But if, disliking the idea of a world resting on compulsion, you ask for an alternative which will preserve our free society, I suggest, as the second of my possible devices for the future, that you might consider what can be done towards a solution of the age-long question of Incentive. And in some ways this is the most urgent of all our industrial problems, for never since the finest trumpetings of the mercantilists, has the clarion-call to work sounded so insistently as to-day. The idea of ' Incentives,' however, seems to involve the acceptance of the view that the natural man does not love work, or that he does not love it or endure it cheerfully, except in moderate doses. Yet apparently this innocuous proposition seems to be one which it is rather dangerous to advance in certain circles. For moralists tell us that we ought to love our work, and tend to think that if we have not yet reached that stage, the fault lies not in man, but in the organisation of labour. As Fourier said in one of his illuminating flashes of vision : ' Morality teaches us to love our work ; let it then know how to make work loveable.' And as you may have discovered in your miscellaneous reading, any suggestion that mankind as a whole have in their make-up a something which leads them to regard work as no more than a second-best way of passing the time, is at times roundly described as a foul slander on our fellow-men. Yet I am unrepentant. I am sufficiently under the influence of Jewish mythology to believe that work is a curse, and I take refuge under the broad wings of St. Thomas Aquinas who somewhere has tersely summarised Man as an animal *laborem fugiens*. What I would reject as a ' foul libel ' would be the suggestion you may also have encountered that Man is as lazy as he dare be without risk of being found

out. Searching my own heart in a desire for a just delicacy of statement, and contemplating a life-long struggle against what Godwin called the ' allurements of sloth,' I would confess for myself that I have been just about as lazy as my self-respect would allow. That, when I come to think of it, is rather a nice formula, tossing the ball back to our friends the moralists.

Work, for the present, then, I assume to be something which soon comes to be avoided, except for a small happy section of the community whose work and whose play merge into each other—the small body of insincere cranks who in *Who's Who* describe their recreation as ' More Work.' We work because we must ; we consent to do more work, because of additional inducements and incentives. But in the new world to which we are moving the question of incentive, of overcoming man's disinclination to work, will of necessity become vastly more difficult, just because the older incentives in their harsher form must become enfeebled as we seek to guarantee security and a reasonable standard of life in all circumstances.

Doubtless the problem of incentives should be considered along with the allied question of the removal of disincentives, if we may lapse into the barbarous jargon of these times. I am not sure that the Authorities realise how powerfully in certain circumstances the present Income Tax arrangements operate to restrict effort. The fact that payments in respect of overtime may bring into the group that pays Income Tax a worker who would otherwise be exempt leads him to assign the whole of the tax exclusively to that portion of his earnings that comes from the extra hours worked. He complains that whereas he has from time immemorial been paid time-and-a-half for work outside normal hours, he is now being fobbed off with three-quarters time : he is in fact being paid at a lower rate for his overtime than for his ordinary day's work. There is of course a catch in it ; but if you discuss the matter with him, unless you are a very good dialectician, he has a fair chance of persuading you that he is right.

Waiving the question of the removal of such ' disincentives ' which I mention merely to complete the picture, it remains true and regrettably true, that the only effective incentives are of a material character, with an appeal to the individualistic and competitive instincts of mankind which we are supposed to be eradicating. You may ring the changes on higher rates of pay and ingenious bonus payments ; you may bring in shorter hours ; but you are still moving on the material plane and appealing in one way or another to the desire of gain and comfort. Nor do you escape (entirely, or at all) this material character by giving better seats at the Opera, or tours to the Northern Capitals, on the *Kraft durch Freude* principle. And as I said, precisely because we are moving into a better world, this problem of incentive must become more difficult : it may indeed turn out to be the acid test of the stability of our future economy. We have the firmest assurances that the Government will in future accept the task of maintaining full employment, though doubtless ' full employment ' may be variously defined. The testing time which will reveal whether the State has at all times and in all circumstances the power to fulfil this undertaking still lies ahead. But if full employment is defined, as it sometimes has been defined, as a world in which there are more jobs than men, then I tremble as in the presence of a nightmare. I am aware that I am again endangering my life : for I have already read the words of ecclesiastical dignitaries, censuring the cruelty of those who would drive men to work under the lash of hunger. One ought to be able to assume in bishops a knowledge of the Book of Genesis. The whole of Man's existence on earth has been a struggle for the conquest of bread. In a world where there are *more* jobs than men, it is fairly obvious that two things will happen. Firstly, and by definition, a number of jobs will remain unfilled, and it is not difficult to say which jobs these will be. Man, *more suo* and *laborem fugiens*, will avoid those tasks which, for any reason, are regarded as unattractive, unpleasant or unduly arduous, though it may quite possibly be that without these a city may not stand. And simultaneously, higher up the scale, employers in desperation will spend their days enticing away the employees of others by the offer of higher wages, so that in the end we may say of wages, in the words of Isaiah (in his metrical version) :

> On eagles' wings they mount, they soar.

It is sometimes instructive to see on a small scale what may later happen on a larger. To-day you have a fairly good illustration of a world of more jobs than men, if you take the market for domestic service. Domestic servants, so far as they survive, having read their Bernard Shaw, make a bee-line for widowers' houses—elderly widowers, with no children about the place. I do not blame them ; I should do the same myself. An elderly widower who spends all his evenings at the club provides a haven of peace compared to anything that can be offered by a harassed young woman with four children. Also the elderly widower is more squeezable in money matters. At present, accordingly, the survivors of the race of domestic servants flock to the houses of those who in many cases

might be better dead, and avoid those homes where the need is the sorest. This is almost a parable of what would take place in a world of more jobs than men ; and the only possible remedy is clear. If there is a danger that certain essential tasks may remain undone because no one is willing to undertake them, the State, even if professedly shunning compulsion, would have to devise means of coercion. And so by another route, you are brought back to compulsion which we have already considered.

While on this question of incentive in this present transitional world, and with men as we still know them, I should like to allow myself a passing observation on two points in partial criticism of the tendencies of our time. Firstly, am I perverse in sometimes feeling that to-day we are tending to be far too impatient of any suggestion of inequality in any sphere of life ? I am still enough of a Saint-Simonian (and for that matter, a Fourierist) to consider that it is only by admitting a certain measure of inequality, something of the nature of a hierarchy, that we shall be able to get anywhere at all, or be able to make the machine march. The only place where there is absolute equality (just as it is the only place where there is absolute security) is in the grave. There is, I think, no harm in a certain degree of inequality, on two conditions : firstly, that the resultant disparity should not be offensive ; and secondly, that there should be no barrier in the way of the somewhat-less-favoured of to-day becoming the somewhat-more-favoured of to-morrow by their own efforts, and (admittedly) by such an admixture of luck as you will never eliminate from life. Life always has been, and always will be, something of the nature of a race ; and the young at least would have it so. But there is not much fun in taking part in, or in watching, a race where in advance the umpires impose handicaps which will effectively ensure that all the competitors will arrive simultaneously at the winning-post. In fact, incentives won't work, unless you are prepared to allow some inequality of one kind or another. And there is this further point. We turned to the possibility of devising effective incentives, in order to avoid compulsion and to preserve our free society. But indeed though Liberty and Equality have been for a century and a half yoked with Fraternity in a curious and uneasy Trinity, nevertheless Liberty and Equality are natural enemies. It is the dilemma which vexed William Thompson for a lifetime—a dilemma from which he never escaped. For if we are to be free, and if we exercise our freedom, we must be free to be, among other things, unequal. On the other hand, if we are to be equal, it will only be because we are forcibily compelled to be equal, and denied the liberty of surpassing our fellows.

The second general point which I would merely mention in all this question of incentive and work is whether we may not have been striving somewhat too zealously and automatically in the direction of a shortening of the hours of work, regarding this as an end desirable in itself and in all circumstances and apart from all other considerations. It is a curious situation that a time marked by unremitting calls for increased activity should also be marked by continual strivings for reductions in the hours of work, and in some cases in the number of days in the working week. I am aware that I am again in danger of seeming to speak with the voice that betokens the heart of the callous slave-driver, and therefore I must tread delicately. I have already admitted that work is, in our professional jargon, a disutility ; but those of us who have intermittently lapsed into work will probably agree that it is not such a loathesome thing as might be imagined from the unguarded talk of men taking their Sunday ease in their inn. It is not nearly so objectionable as, for instance, idleness spent in boredom. I am merely concerned to suggest a doubt as to the correctness of the assumption which we seem to make that every reduction in the hours of work must of necessity be a blessing in all cases. Doubtless at this point we pass the invisible frontier between Economics and Ethics ; but I would like to suggest that the question whether a diminution in the hours of labour is or is not a blessing depends in part on the use made of the additional non-working hours. Ideally I would like a reduction in the hours of work to march hand-in-hand with a more intelligent use of the hours of leisure. A mere increase in the number of hours when a man does not know quite what to do, when he is merely a nuisance about the house, is probably a curse rather than a blessing. For assuredly, leisure unless it is filled with some occupation that almost assumes the dignity of work has, like other things, diminishing utility.

I have offered you compulsion ; but you say that if possible you would prefer to retain your freedom. I have suggested that you endeavour to solve the question of incentive in a free society ; but you tell me that there may be something illogical in our new society in relying on incentives which, at their worst, as in *Taylorismus*, are unlovely, and which are of necessity material in their nature, with an individualistic appeal, inevitably differentiating if not dividing men. The third course—I promised you three—is to enrol yourself frankly among the followers of Lenin (for this purpose) and wait in faith for the

emergence of a better Man, for the universal prevalence of a higher order of morality than that now to be found among us ; when in consequence the worker will no longer (the words are Lenin's) ' calculate with the shrewdness of a Shylock whether he has not worked half an hour more than another, whether he is not getting less pay than another ' ; when ' the necessity of observing the simple fundamental rules of human intercourse will become a habit.' Admittedly this is a long-term policy and a long-term hope, and postulates, in the words of Lenin, 'a person unlike the present man-in-the-street.'

How far we can make ourselves fit to live in a better world by evolving a higher type of morality is presumably not an economic question. Nevertheless I may not omit it to-day, because in the past—Louis Blanc is a useful example—it has so often been hoped that our economic difficulties would be solved in some such way. Nearly all our ardent reformers have confidently trusted that in a better world where all things are held and operated on behalf of the community, a higher order of morality would prevail. When workers could feel that they were working for their fellow-workers and not merely for the gain of another, they would adopt a new code of behaviour. Admittedly it will be a slow process, even on Lenin's own showing ; and therefore perhaps we should not be impatient. I doubt whether so far any one who in the past would have swindled the Railway Company has started like a guilty thing upon a fearful summons on observing that the letters ' L.N.E.R.' had been replaced by ' B.R.' Also we have made doubtful progress towards industrial peace ; for apparently employment by the State or by a National Board does not necessarily mean bliss and content for all concerned. There is a devastating sentence in the second Annual Report of the Coal Board. It tells with restrained gratification that there were no official strikes during the year ; ' but,' it adds, ' there were 1,635 unofficial strikes where men stopped work in defiance of their Union.' One thousand six hundred and thirty-five ! One is reminded of the love affairs of Don Juan : ' And in Spain, a thousand-and-three.' Here surely, in the cold statistics of the Coal Board is an illuminating figure significant of the economic and psychological friction prevailing in our society. It will assuredly take some time before we are all prepared to crucify Self to such an extent as will qualify us for admission to Lenin's heaven.

And so I leave it—somewhat inconclusively, like certain modern composers who end without even asking a question, but merely because they have exhausted their time or their paper. Compulsion, shall we say, is detestable and ultimately immoral. Incentive is essentially a species of bribery, relying on the competitive instincts and leading us back to what some would have us regard as the jungle of individualism. I am all for the evolution of a better Man, and for that matter of a better Woman ; but it is a slow process for frail creatures whose years are three-score-and-ten. Perhaps in the end there *is* a conclusion, though it is not an economic one. It is that before we can be trusted to live in the New Jerusalem, we must first of all be fit to walk the streets of the New Jerusalem. And as applied to our transitional times, I would suggest that despite all superficial appearances, the New World into which we are moving is not going to be a world which will make everything easy for everybody by giving everybody everything. If it is to work, it will be a world which will make vastly greater demands on every one. It will demand that most difficult of all things to attain, that plant of very slow growth, a higher standard of public and private morality in all things, and in particular the suppression of Self. For Socialism is parading under a false name, unless it means an order of things in which we forget ourselves in our zeal for the good of society and of our fellows, and in which speculation as to our place in the queue is the last thought that occurs to us. And it is not I, but Lenin, who says so.

SECTION G—ENGINEERING

BRIDGING THE GAP BETWEEN SCIENCE AND INDUSTRY

ADDRESS BY

SIR ARTHUR P. M. FLEMING, C.B.E.

PRESIDENT OF THE SECTION

TYNESIDE has long and very eminent associations with some of our greatest engineering and other industrial activities. Newcastle is the centre of one of the oldest and finest coal mining areas in the world, and there are references to the mining of coal here as far back as 1180. In the House of Commons in 1642 Newcastle was spoken of as 'The Nursery of Shipping.' In shipbuilding and ship-repair Tyneside is pre-eminent—the first iron ship was built on the Tyne, and the pioneering work of welded construction of ships was carried out here. George Stephenson built his first locomotive, and opened engine works, in Newcastle. William, Lord Armstrong, founded the famous engineering, shipbuilding and armament works at Elswick. Joseph William Swan did much of his early work on the first incandescent electric lamp, particularly in regard to the filament, at his home at Low Fell, near Gateshead. Sir Lowthian Bell, who developed the Cleveland ironstone field in the middle of the nineteenth century, is considered to have been the founder of scientific blast furnace practice. Sir Charles Parsons made his first turbine in 1884, and the famous engineering works which he set up has supplied prime movers for ship propulsion and for the generation of electrical power all over the world. The influence of the steam turbine on the economic generation of electrical power has been far-reaching and one of the outstanding milestones in engineering progress. The Newcastle and District Electric Supply Company was the first public supply company to employ three-phase alternating current, which they introduced in 1900. In 1904 this company constructed the Carville station employing a 3,500 kW. axial flow turbo-alternator set having more than twice the capacity of any set then installed. For many years the Carville 'B' station held the record for being the most economical power station in the world. When later, in 1930, the company opened the new Dunston power station more records were broken by the machines installed—three 50,000 kW. turbo-alternators, the last to be built under the direct supervision of Sir Charles Parsons. Another famous Tyneside engineer was C. H. Merz, consulting engineer on electric traction to the Newcastle Electric Supply Company and responsible for the electrification of the North Eastern Railway Company's line in 1903 and many other electrification schemes in all parts of the world.

Tyneside has always been research-minded. As long ago as 1813 a Sunderland Committee was formed to attempt to solve problems in mining. They contacted Sir Humphrey Davy on the question of lighting, and he produced a miner's safety lamp in five months. In more modern times, from 1922 to 1927, the Newcastle Electric Supply Company provided power facilities for the Electrical Research Association's circuit breaker research. This led to the development of the Wedmore and Whitney patents in 1927 for gas blast circuit breaker construction which attracted world-wide attention.

Mention must be made of the North East Coast Institution of Engineers and Shipbuilders. Established in 1884, this Institution has always been keenly interested in the training of young engineers and its reports on the training of apprentices have had wide influence. Many classic engineering papers have been read before the Institution.

Many of the great achievements of the past as, for example, the work of Parsons, have resulted from the patient and persistent work of inventors and industrialists based on the fundamental discoveries of the scientist, and such discoveries will always be a starting-point for new industrial enterprises. To-day, however, the function of science in relation to industry is not only to provide a new idea which may have industrial application but also to provide the means whereby that application can be most completely achieved. The achievement will always involve many industrial problems, and it is in the surmounting of these problems, whether they be

associated with men, materials, machines or money, that is the modern conception of the application of science to industry.

The function of industry is to make natural resources available for the use of man. This may be by the extraction of minerals from the earth, by the generation of power from flowing water, by the combustion of oil or coal, or by the nuclear disintegration of certain elements, or it may be by the conversion of raw materials into a finished product through a sequence of manufacturing operations—each of which may be an industry in itself. Industries become closely interrelated but all depend on the engineer for power, machinery, transport and communication. The tools and processes of one industry often assist the progress of others, and it is economically important to ensure that every process employed is carried out scientifically—that is to say, by the elimination of waste of time, effort and materials. Industry never remains static—it marches forward on an ever-widening front, and the rate of its progress depends on two factors—*the acquisition of new knowledge and its effective use.*

In earlier days new knowledge resulted from the accumulated experience of workers and was stimulated by pride in achievement and by competition. To-day, new knowledge, to which there is no finality, is derived increasingly from organised scientific research. In the past a new scientific idea usually arose from the contemplation of natural phenomena by scientists working more or less in isolation. To-day, to an increasing extent, discoveries are the result of team work.

Facilities for research in pure science have greatly increased in recent years. In the Universities post-graduate research is carried out more extensively, some of the large industrial companies have established long-term research laboratories and the Research Associations have, in some instances, directed some of their facilities to pure science investigations.

With rising costs of labour and materials, most of such fundamental research is becoming increasingly expensive and, depending on its character, the cost may be prohibitive. Notably is this true of research in some phases of nuclear physics—a field of science which may ultimately be of great significance to the engineer in connection with the development of nuclear energy for industrial purposes. The particle accelerators used in this field of research may weigh thousands of tons and require buildings of cathedral-like proportions for their housing—conditions which would have staggered physicists only a few years ago. The magnitude of the operations involved in such types of fundamental research emphasises the need for co-operation to avoid wasteful overlapping of expenditure.

The first stage in turning new scientific knowledge to industrial account is carried out by *industrial* research. This requires the provision of laboratory facilities and of staffs trained to interpret the significance of scientific discoveries in the light of their possible industrial applications. Such research may lead to the development of entirely new industries and assist in the rejuvenation of existing industries by effecting improvements in tools, materials and processes. It may also provide new research instruments such as the electron microscope and the mass spectrometer, and techniques such as the new methods of perception and research afforded by the use of radio-active isotopes.

Particularly important to engineering are researches in metallurgy resulting in, for example, materials of improved creep qualities so essential to the advancement of steam turbine practice. In gas turbines considerable gains in efficiency and output would result if alloys that will stand the hot gases at temperatures only slightly in advance of present practice were available. Improvements in magnetic materials and in the quality of many insulating materials would be greatly prized by the electrical engineer.

Industrial research has been practised by certain of the large industrial concerns in this country for nearly half a century. Great Britain is, however, apart from a few large organisations, the home of small industrial units which are unable individually to bear the cost of extensive research laboratories, and this led to the establishment, over thirty years ago, of the Industrial Research Associations which now number nearly forty. The function of these Associations is to undertake research in problems common to their respective industries and make the results available to all member firms. An important service they have rendered has been the bringing together of competitors in the same industry who have pooled their hard-won knowledge and experience for mutual benefit. This pooling of knowledge does not preclude competition between the member firms but forms the starting-point for a more complete exchange of confidences. It represents a form of raw material which each user turns to account according to his skill and experience—some with more success than others.

While in Great Britain there is an increasing tendency towards co-operation in industrial research, particularly in the nationally sponsored activities such as the Research Associations, it is interesting to note that there is not the same evidence of co-operation in the

United States where, apart from the laboratories of individual firms, institutions on the lines of the Mellon Institute undertake research problems for individual manufacturers.

When an idea has been tested experimentally in the industrial research laboratory the next process in the transmutation is to see whether it has marketable possibilities, and whether manufacture can be carried out economically. Many apparently promising ideas emanating from the research laboratory may be found to be non-competitive on account of cost and difficulties of production. In this stage it is often found desirable to set up pilot plants in which production problems can be solved and products turned out on a scale sufficient to enable quality and competitive marketable values to be determined. The personnel concerned with this phase must, in addition to sound scientific knowledge, have experience in the various aspects of commercial production. In many respects, this is one of the most critical phases in the transmutation process.

Assuming the stages thus far described have been passed through successfully, and that a manufacturing organisation exists to provide the requisite facilities and has the courage to go forward, the transition from the development stage to commercial production presents a wide range of new problems. For this reason it is generally found best to make the team which has successfully nursed the new product through the development stage, responsible for getting it into production and seeing it through initial difficulties to the point where it is accepted and absorbed into the orthodox manufacturing programme.

With the facilities for production and distribution established, the important problem is to keep the product and its production up to date. This involves the application of all relevant new knowledge as it becomes available. For example, new tools, which are an improvement on those being used, may appear, new materials of construction may come forward, and new processes may find fruitful application. The experience of users must be taken into account and statistical analysis of the significant variable factors must be made. These variables relate to such matters as quality of raw materials, quality and rate of production, etc., which though often minute in themselves indicate the steps which must be taken to improve efficiency. The effects of troubles in production arising from these variables are often misleading, and the only way to deal with such problems satisfactorily is by the investigation of the cause—for this purpose the technical knowledge now so freely available is the most effective tool.

The importance of reducing the time lag at every step between the discovery of a new idea and its ultimate conversion into a usable product cannot be over-estimated. Economically it results in securing markets ahead of competitors, and as has been rightly said, ' it is as important to be first to apply new knowledge as to have been the discoverer.' In this connection it is significant that industrialists in America excel in the rapidity with which they carry into effect any new project. The value of time in making a new idea marketable seems to be much more acutely realised there than here, and the reactions in normal times of the American industrialist in this respect are often not far short of those obtaining under war-time stress.

The need for extending the facilities for research, whether in fundamental science or industrial application, requires little emphasis. On the other hand, there must, of course, always be an economic balance between the cost of research and the national economy in respect of money and personnel, and there is a serious weakness in our national economy due to the fact that the knowledge available from research is by no means always fully used. With the small concern this may arise from lack of technical staff able to assimilate and turn to account new scientific knowledge or it may be due to reluctance to risk capital in the plant and equipment necessary to implement the new project.

The personnel involved in the efficient use of new knowledge covers the whole range from the apprentice entering industry at the school-leaving age up to the highest ranking officers, and there is need, not only for fundamental training in theory and practice, but continuous alertness to apply all new knowledge to the ever-changing conditions.

At the professional level educational facilities for engineers are well established in both Universities and Technical Colleges, but it is important that the teaching staffs should have greater opportunities for maintaining contact with industrial practice which is expanding and changing at an accelerated pace. Some firms have provided ' Summer Schools ' in their works to enable teachers to familiarise themselves with up-to-date practice, but from time to time longer periods of, say, six months to a year, in engineering works or industrial research laboratories would be really invaluable to both teacher and industry.

Much attention is being directed to the study of Further Technological Education and to the facilities for making scientific and technological knowledge which is too new to be included in the normal curriculum quickly available to young engineers who have already reached professional levels.

So much has been written about the education and training of the various grades of personnel that repetition here is unnecessary, but particular attention might be drawn to the training of the industrial research worker, the development engineer, and the higher management personnel. Some laboratories absorb their industrial research workers at graduate level, while others prefer Ph.D.'s, and some provide the graduate entrants with training in the works before employment in the laboratory. Again some firms arrange for the graduate who has shown promise in the laboratory to return to a University for advanced training in his particular field, and by so doing he will usually advance to a higher degree. Since it will be the development engineer's function to negotiate the difficult transition of a new project from the laboratory into the works he should preferably have had some research experience as well as sound practical training and knowledge of works practice and factory processes.

In recent years much attention has been paid to training for management, and facilities are now available for the classroom instruction of young engineers and others in the subjects which constitute ' Management.' Previously such knowledge was obtained by the lengthy and laborious process of individual experience. Knowledge of ' Management ' subjects is, however, no substitute for the quality of leadership which is the first essential in a successful manager.

In connection with engineers at the graduate level comparison is often made between the conditions in this country and in the United States—to the disparagement of the former. It is not, however, always realised that the term ' graduate ' in the United States and in this country may signify quite different educational levels, and while there are notable exceptions, broadly speaking the educational level at entrance to the American Universities is lower than in this country. No doubt, this to some extent accounts for the very large numbers of students in the United States who pursue post-graduate courses, some of which are of a very high standard.

From an engineering point of view the problem of practical training is quite different in the two countries. Here we insist on practical experience as an essential part of an engineer's training, whereas in the United States very little actual practical work on the bench or in the machine shop is included.

The very large number of engineering graduates in the United States, many of whom would be considered here to have reached only the technician, as against the professional, standard, does provide an asset of great value to industry, and enables every production activity to be undertaken by men who possess a knowledge of underlying principles. In the United States there are practically no part-time educational facilities such as are available in this country through the National Certificate system, whereby young people entering industry at the school-leaving age can, if they have the enterprise and initiative, acquire a high level of technical education, although they lack the advnatages of the broader general education which all University courses provide. The part-time courses in spite of their limitations provide a most valuable supply of both technicians and professional engineers. The system also affords the means of revealing latent talent which represents a most important national asset and should be developed to the utmost.

The economic importance of utilising science in promoting new, and assisting existing, industries, needs no emphasis. The competition for world markets will grow keener and we need to turn to account all our assets of skilled craftsmanship and technical and scientifically trained personnel to maintain a lead over our less experienced competitors. Our needs in this respect are urgent and in his Presidential Address last year Sir Henry Tizard pointed out the importance of employing to the utmost the knowledge we already possess but do not utilise. He also expressed the view that our standard of technology is lower, and our proportion of highly scientifically educated people in executive control of industry less, than is the case with some of our international competitors. In this connection it should be remembered that in this country many of the most able men of past generations have—in line with age-old tradition—received a classical education. The rising generations contain a far greater proportion of those educated in science, who in turn will proceed to the highest positions in management, and thus there will be an increasing number of scientifically trained executives.

An outstanding example of bridging the gap between scientific discovery and its practical application was provided in the war by the production of the atomic bomb. The starting-point was the discovery of the electron fifty years ago. This discovery inspired physicists the world over to study atomic structure, and fundamental research in this field led to the discovery of the means of producing nuclear disintegration. Then followed the discovery, just before the war, of uranium fission and the chain reaction which enabled the release of a vast amount of energy from the nuclear disintegration of the uranium isotope of U-235.

Carrying this discovery into effect involved extensive research activities both in physics

and electrical engineering, and ultimately led to the construction of plant and equipment on an unprecedented scale. This work, conducted in America with characteristic vigour and irrespective of cost, achieved results which are well known. The knowledge on which this achievement was based is being extended by continued post-war research on a very large scale, and this, while primarily conducted from the point of view of national security, will eventually produce knowledge of great benefit to mankind.

The post-war research under the control of the Atomic Energy Commission is conducted in a number of important laboratories in different parts of the United States each specialising in a particular aspect of the subject. For example, the Knoll Laboratory is studying nuclear disintegration with a view to its being applied to the production of industrial power, and the Radiation Laboratory of the University of California is concerned with particle accelerators—the newest of which when completed will weight about 4,000 tons and produce 6,000,000,000 electron volts. Certain of the laboratories of the Atomic Energy Commission are directed by industrial organisations. Training schemes have been established for instruction in the new techniques involved when dealing with products and processes that are dangerous to health, and in the new laboratory and engineering constructions that these atomic energy developments require.

The economic production of industrial power by nuclear disintegration does not appear likely to be successful for a long time, but already great benefits have arisen from the war-time development of the atomic bomb. One by-product, radio-active isotopes, has provided a new and extremely sensitive means of perception of great potential value in almost every field of industrial research. Arising out of the whole of this atomic bomb development, and apart from benefits that may come from industrial power and the use of radio-active isotopes, great potential benefit will accrue to the United States because large numbers of scientists are gaining experience in the practical applications of nuclear physics, and will be able to turn to industrial account new discoveries that may be made in this field of science.

It will be the shortage of adequately experienced scientists that is most likely to delay corresponding progress in these new industrial fields in Great Britain.

National security demands the maintenance of our industry at its maximum efficiency, and this depends, to an increasing extent, on the scientific personnel. Outstanding leaders in pure science are rare, and the demand in this country for industrial scientists and technologists of the highest calibre already exceeds the supply. In order to ensure that the available personnel is employed to the greatest advantage, a much greater degree of co-operation between the scientific work carried on in Government and industrial laboratories is necessary.

It must be very pleasing to all members of this Section that our meeting this year takes place in an environment so richly endowed with engineering associations, and one which has provided many striking examples of bridging the gap between science and industry.

SECTION H—ARCHAEOLOGY AND ANTHROPOLOGY

THE PLACE OF ARCHAEOLOGY IN OUR NATIONAL EDUCATION: A SURVEY AND A PLEA

ADDRESS BY

MILES C. BURKITT

PRESIDENT OF THE SECTION

THE main theme of my presidential address is the suitability of archaeology—especially in its earlier stages—for inclusion as a major subject in our national education. To introduce and elaborate this I shall have to try, as it were, to stand back for a moment and review the subject as a whole. Such a survey should be made from time to time and seems to me both useful and necessary. Too often the scientist is so occupied with adding just one more brick to the edifice of knowledge that he fails to see the shape and layout of the structure he is building—or the places, some of which I shall hope to indicate, where special work is needed. Time being limited I shall have to confine myself to touching on certain salient points which seem to me important in the elaboration of my theme, but such a survey I feel should rightly be the task of the specialist and not left, as is so often the case, to the 'scissors and paste' man, who necessarily cannot have as part of his being the atmosphere of the subject which comes from long and single-hearted study of it. Moreover it will be found, as I have suggested, that when we stand back and consider what we know about the early history of mankind a number of problems still present themselves as unsolved. At first sight these almost twin motifs, (i) suitability of the subject for national educational purposes and (ii) a review of important points with a stress on some unsolved ones, may not seem to have much connection. But I hope to convince you that this is not so: the foundations of our subject are now well and truly laid, but the accessibility to the amateur of the problems still needing elucidation is just one of its fascinations for the young. One does not need to be a mathematical or linguistic or scientific specialist to be able to appreciate the problem I shall touch on.

My own qualifications for daring to tackle my educational theme are perhaps somewhat unusual in that while I am a prehistorian by profession and have lectured to successive university students for the last thirty years—incidentally the best way of steeping oneself in and learning one's subject!—I have, for the last ten of them, been a County Councillor and member of the Local Education Authority in Cambridgeshire, the home of the famous village colleges. Indeed I am Chairman of the Governors of one of these latter, as well as a member of the Primary and Secondary Education sub-committees, and Chairman of the Further Education sub-committee, of the County Library and Children's Committees of the County Council. I can claim, therefore, to know a little about prehistoric archaeology and also about our national educational system, at least as far as it applies to my own part of the country.

I shall be suggesting as we go along that the early story of mankind has a *double* claim for inclusion in the curricula of our schools, for not only is the subject matter itself of fascinating interest and importance, dealing as it does with the beginnings of human history, but also the methods employed by the archaeologist to elucidate the available information, though readily understandable by even the B streams in our secondary modern schools, do involve logical processes of thought which are extremely well suited for training the young mind to think, and incidentally are frequently adaptable for practical demonstration. As an example may I mention a palimpsest MS. Even a C stream lad might be induced to realise that the writing underneath must be older than that which covers it and could prove this to his satisfaction in a very practical manner with his own pen and ink or chalks and paper. Yet at bottom he has invoked the geological law of superposition upon which the whole of our knowledge of the earth's history is based. It is true that if, leaving the palimpsest, he experimentally builds up a section with gravel and stone and sand he can discover occasions where this fundamental law might not seem to fit the facts, but that will merely result in his learning that in citing it he must never

forget to include the words, 'should there have been no subsequent disturbance.'

Now for my survey. Archaeology is a very wide subject and the archaeologist has to cover a great deal of ground. His interests start with the first appearance of man and continue well into the Middle Ages and beyond. In the earliest times the archaeologist reigns supreme ; he alone can tell the story of that remote past. Later, on the other hand, he is but the handmaid of the historian, adding here and there factual bits of evidence and checking from time to time the information obtained from written records. It has become usual, as you know, to describe the early periods as prehistoric and to call the workers in this realm Prehistorians. The later stages are dealt with by the Antiquary. No convenient name has yet been invented for the intervening periods, from the time when men had just learned the possibility of food-producing and even simple metallurgy, although the terms Proto-history and Proto-historians have appeared in the literature. I shall be more concerned to-day with prehistory and early proto-history than with the labours of the antiquary. Actually the antiquary was in the field long before the prehistorian. One has only to remember the numerous excavations that were undertaken in connection with medieval buildings and so on. Dugdale in the seventeenth century was our first scientific antiquary, but he learnt much from his predecessor Camden as to methods, etc. But prehistory although a new subject has now reached a respectable maturity, and it would seem that its foundations have been well established ; its dicta are no longer matters of guess-work. There are solid irrefutable arguments to back up the main outlines of the story, although, as I have said there still remain some fascinating problems to solve and much local detail to fill in.

The first stone implement was found as long ago as 1690, but was not recognised as anything more exciting than a Roman object, and it was not until a hundred years later, in the 1790's, that the study really commenced with the work of John Frere at Hoxne. I often feel that prehistorians forget that it was Frere, an Englishman, who first started the ball rolling. So much excellent work has been done subsequently by people abroad, more particularly by the French, that we sometimes forget that John Frere of Roydon Hall, near Diss in Suffolk, was the Father of Prehistory. It was he who first noted the occurrence together in the same geological horizon of knapped flints and the bones of extinct animals, from which he drew the logical conclusion that as there had been no subsequent disturbance the flints and bones must have been contemporary. A simple fact obvious to a secondary modern school boy or girl—yet fraught with importance to all subsequent prehistorians. Hoxne is a little village lying just south of the river Waveney not far from Eye, a place much valued by composers of cross-word puzzles. It was at Hoxne that Saint Edmund was tied to a tree and transfixed with arrows. This, as well as the brick-earth pits which Frere studied, would seem to make this little village a place of some interest and worthy of a visit ; but I do not gather that it is much disturbed by the outsider. As I have said, the mantle of John Frere passed for many decades to a succession of brilliant French investigators. Some of these like Boucher de Perthes, Lartet, Piette, and Commont were concerned with investigations in the field, others like Gabrielle de Mortillet were more interested in the systematisation of the information obtained by the field investigators. As these were mostly Frenchmen and working in France, the well-known scheme of successive cultures which was evolved related only to parts of western Europe. Unfortunately it was applied universally. And here I want to make one of my first points. All professional prehistorians are, or should be, well aware that the Mortillet succession of cultures—Chellean, Acheulean, Mousterian, Aurignacian, Solutrean, Magdalenian, etc.—does not hold good in its entirety much beyond the confines of France. Yet again and again the excavator living in some far-away region of Africa or Asia tries forcibly to describe his local succession in terms of this western European table. The result has frequently been chaos, and the applying of the same name to what have subsequently been found to be similar but entirely distinct cultures. It cannot be too strongly urged that in the present state of our knowledge prehistorians in widely separated areas should work to establish their own local cultural successions and not attempt to force their finds to fit into a purely European scheme or to determine the exact relationships between their own and those of other areas until the finding of enough intervening links proves their points beyond contention or their own knowledge of whole-world problems qualifies them to risk a hypothesis.

This brings me to one of the unsolved problems of prehistory to which I want to draw your attention. It is a fact not easy to explain that the general pattern of cultural development as illustrated by the stone and bone industries of palaeolithic and early neolithic dates that have survived is so very similar in many very different parts of the world, and yet the local cultures concerned frequently appear to have no connection wit

those of other areas. It seems to be established beyond doubt that, where they occur, simple core-tool industries preceded flake-tool industries, and these in turn are succeeded by others where the implements tend to be made on blades, while later still we get the appearance of a composite tool, that is a tool in which the haft is made of one substance suitable for the purpose such as wood or bone while the business parts—harpoon barbs, knife-blades, saw-teeth, etc.—are made of another substance such as flint or obsidian, which is sharp and gives a good edge but is less suitable for hafts on account of its weight or brittleness. At times when such composite tools were being made vast numbers of these component pigmy flints abounded forming what is known in Europe as the ' Mesolithic spread.' Finally the technique of grinding and polishing stone is discovered and we enter the neolithic period proper. The question is why and how does this general pattern in the tapestry of early human cultural development seem to hold good everywhere when it would appear, at first sight at any rate, that, say, the flake-tool cultures of one area are unlinked in time or space with those in another ?

It is true that in the earliest period evidence can be adduced to show close cultural connection between the lower palaeolithic Chelleo-Acheulean (Stellenbosch) culture of South Africa and those of east Africa, north Africa, and western Europe. For the evolution of the industries belonging to this culture-group in these wide-flung areas is so similar as to be almost identical, even though the climatic conditions and the materials available for tool-making were not the same : a fact which is difficult to explain other than by the theory that the Chelleo-Acheuleans were by nature a very conservative folk, and that they slowly moved about Africa and western Europe as molecules do in a liquid, so keeping uniform their evolutionary development. None the less this is an extraordinary phenomenon if you pause to consider its implications. For, as a rule, different climatic conditions and different materials for tool-making purposes engender different industrial developments. Even in our own day when the dead hand of uniformity presses ever more heavily upon us, and modern transport either of views, of persons or of goods has made close intercourse between peoples of the same stock a commonplace, yet no one would suggest that the cultures of the Flemings, the English and the Americans are in any way identical. But the changes in the Stellenbosch industries of the Cape can be matched step by step in east Africa, north Africa and in western Europe. On the other hand, it is impossible to think that the succeeding flake-tool industries of Africa are directly connected with those of Europe, nor indeed are the pigmy ' spreads ' which eventually succeeded them after the sketchiest of blade industries. There is indeed every reason to deny a close cultural connection between the Tardenoisean of Europe and the Wilton of South Africa. It is true, of course, that heavy core-tools are clearly necessary in forest areas, whereas the lighter flake-tool lance-heads would be vital for the life of a steppe people. It is always necessary to take into account the climatic milieu. For instance, if a people are going to use composite tools it follows that at the time softwood forests must have existed to provide the hafts : and softwood forests in western Europe would not have flourished during the glacial maxima of quaternary times. But it has always seemed to me significant that during interglacial periods, more particularly the great interglacial that lies between the Mindel and the Riss glacial maxima, when the climate would have seemed to be entirely suitable for forest growths of various kinds, pigmy flints are rarely found. Indeed the only period in the pleistocene epoch when this idea of a composite tool seems to have been thought of was towards the end of the very cold period which obtained in upper Aurignacian times. The Gravette knife-blades characteristic of this late Aurignacian culture would seem to have been hafted, possibly in grooved bones (although no actual evidence for such hafting has survived) for many of these knife-blades are very small —too small one would think to have been used directly by hand.

Two schools of thought exist about this problem. The latest South African opinion seems to favour the direct evolution of the Middle Stone Age complex from the Stellenbosch and, later, the development within it of some blade tools, although those in South Africa never blossomed into the rich blade and burin industries we have in the north. I do not mean to say that the South Africans are entirely parochial and merely thinking of the Union itself. They are considering Africa as a whole and suggesting that the general pattern I have mentioned above is the result of an evolution of the earliest cultures and not of the imposition by invasion or more peaceful contacts of new ideas from outside the continent. They base their conclusions on a careful typological study of the industries in question. Far be it from me to deny their conclusions in their entirety. Indeed, with much that they suggest I actually agree. But I would like to sound a word of warning. I have been at this job now for nearly forty years and one of my particular interests has been typology and

technology. But the longer I work in these fields the more cautious I become in accepting as certain the results obtained by their study alone. There are only a few ways in which you can smash up large lumps of rock. A perfectly reasonable one is to fracture the lump by a swinging blow, hitting it against a fixed 'anvil.' The result is a flake showing the well-known Clactonian technique, the single faceted striking platform inclined at a high angle to the main flake surface. But when such flakes were found in Uganda we were informed by prehistorians on the spot that the Clacton culture and industries, which were first recognised near that well-known seaside resort in East Anglia, existed, too, in the middle of Africa! How they got there was not specified. In fact, of course, there is no connection between these African finds and those in East Anglia, the latter indeed only occurring rarely in so close a neighbourhood as north-eastern France.

May I give you another example of this kind of danger from another field of prehistoric study, that of the art, where equally typology and technology have to be employed? In southern Spain during the Copper Age (but not elsewhere in Europe) certain characteristic rock-shelter paintings were made. They consist for the most part of figures of animals and human beings, the treatment being highly schematic and stylised. Various conventions for representing the human form can be noted, the anchor, the hour-glass, etc., etc. Outside Spain there do occur a number of rock-shelter paintings of human beings represented in the same way—apart from those in Morocco which may be considered to be geographically in the southern Spanish area. I am thinking particularly of some in the Cordova province of the Argentine where certain of the figures are very similarly conventionalised. I think you will agree with me that in this case it is impossible to postulate the slightest cultural connection between those two groups. Actually there are comparatively few ways of representing the human form in a conventional manner, and it chances that the Copper Age folk of south-west Europe and those at some unknown date in this province of the Argentine happened to make use of similar conventions. Thus I venture to suggest that a too detailed typological study unchecked by other kinds of evidence can frequently demonstrate almost anything one wants to prove. As regards the Middle Stone Age complex in South Africa being entirely derived from the Stellenbosch, I shall be happier to accept the theory when evidence can be adduced from skeletal discoveries to show that the same type of man made those two different types of industries.

The other main school of thought to account for this transcontinental similarity in the pattern of Stone Age evolution depends on the diffusion of these several cultures from their climatically favourable areas of origin into other areas whose climates became favourable to them and unfavourable to those they had previously nurtured and is, of course, equally fraught with difficulties. Locally I dare say the diffusionist theory may well be the correct one, but I do think that it is extremely difficult to see how the Levallois culture of northern Europe ever reached South Africa—the situation is quite different from that described above in the case of the Chelleo-Acheulean—although my South African colleagues will use the name of a suburb of Paris (Levallois) to describe flake industries that occur in their lovely country!

I know little at first hand of the Stone Age succession in south-east Asia stretching from Burma down through Malaya. The basal tools although derived ultimately from pebbles seem to be very different from *coups-de-poing*, and the culture cannot therefore properly be called Chelleo-Acheulean. Yet as far as I can make out, and the discoveries are as yet very meagre, the general pattern still obtains even in this different and distinct cultural region: it certainly does in India. Why and how the industrial development should so universally be core-tools, flake-tools, blade-tools, pigmy flints, polished axes, remains indeed a thought-provoking problem. It does not affect our work as far as our local areas are concerned, but when we consider prehistory universally we find ourselves confronted with this immense question, for whether you support the diffusionist or local evolution theory the implications in terms of human activity on the surface of the globe are astounding and entrancing. If we are diffusionists (and, as I have said, I am for the earliest periods) are we to visualise a race of hand-axe humans in personal contact from East Anglia to the Cape and India? In what language or by what other means did they communicate with each other? This is only one of the legitimate speculations we must make. How thrilling to tell the senior children of such an unsolved problem. If we are local evolutionists must we consider that there is some all-compelling 'geist' in our human race by virtue of which one discovery has led to another in a similar way, though independently? For the moment I think we can only hope that when more knowledge is available about the main intervening areas the solution will become apparent. Perhaps it will be found that some slow change in world conditions hitherto not appreciated may have been, in part at any rate, responsible.

But I have taken too long over this one point which has lately intrigued me so much and must get back to my survey. The prehistorian is concerned with origins—the beginnings of man himself, the occurrence and developments of the first industries, in or outside the de Mortillet scheme of classification, and the first appearance of such activities as painting and sculpture, burial customs, and the like. These latter two activities differ somewhat from the former in so far as they help ever so slightly to lift the veil and let us guess at the thoughts and ideas of these distant forerunners of ours. Of course we can gain much information about them from their skeletons and from their industries that have survived, though it must always be remembered that only those parts that *could* survive remain to us. We must not forget, for instance, that when the climate was suitable and trees abounded many tools may have been made of wood and would have perished. Indeed, we should not judge of the importance of a culture merely from the relics we have to study. What would seem to us a poor industry may have been rich in tools and weapons made of perishable materials, and vice versa. But we do undoubtedly glean information as to Stone Age man's habits and mode of life from a study of his industries in the same way as we get knowledge of a creator, whether indeed a mere man or God Himself, by a study of the things he has created. But in the case of the art and of the careful burials we seem somehow to get into more intimate contact with the persons themselves and their feelings. For a moment, as it were, we leave the kitchen and join them round the fire after supper. Unfortunately, as you know, we have found no evidence of art before upper palaeolithic times, and then only in very restricted regions. It appears with the Aurignacian blade and burin industries of western Europe and perhaps, too, in southern Rhodesia, but it is rare to find it outside the first area named. Careful burials of the dead, generally with simple funeral furnishings, are found at a still earlier period. It has always amazed me that chinless Neanderthal man with his low receding forehead, great eyebrow ridges, shambling gait, and (judging by his endocranial casts) low mental capacity—any schoolmaster present would have had to place him in a 'C' class or indeed in a special school—should have developed ideas which necessitated the careful interring of at least some of his dead; and, incidentally, only some of them, or surely we should have found more examples, together with tools and weapons such as the corpse might be expected to want where he had gone or should he ever return. It is clear that the fact of burial involves ideas about life and death—however elementary. Animals do not bury each other—another point to make the older children think.

Having come now to later Palaeolithic times with the Aurignacian culture and its marvellous art, we are again in a field for further work. The study of the art, more particularly the cave art, still awaits an artist keen on the beginnings of his craft who will tell us something about it from the artist's point of view rather than from that of the quaternary geologist. Any representation of a three dimensional object in two dimensions necessarily involves conventions, and this is just as true of a naturalistic art as of one that is schematic or stylised. But such a study as I suggest cannot properly be done merely with the help of reproductions. It would be necessary for the artist-student I envisage to spend long periods in the caves themselves to get his eyes accustomed to seeing what is there and then to study the way the artists worked and the rules they must have been taught for the practice of their craft. Last year I visited Lascaux, the newly discovered French cave full of late Aurignacian paintings and engravings which was found during the war near Montignac, a small townlet some thirty miles above Les Eyzies in the Department of the Dordogne in France. Unfortunately the cave has now been adapted for tourists and will doubtless, until the paintings there vanish, prove a gold mine to the inhabitants of the place. Indeed, I advise any of you who have heard of this wonderful discovery to see it without delay; for, in spite of all the precautions taken, I have little doubt but that the changes of atmosphere, of temperature and humidity consequent on the passage of thousands of human bodies through the very small cave, not to speak of accidental mechanical damage to the paintings, will rapidly cause them to deteriorate. The combination of conditions which have permitted the preservation in all their pristine freshness of these cave paintings during fifteen thousand years at least is one that has rarely obtained. The entrance must have been blocked not long after they were made and nothing can have happened subsequently to disturb them in any way. But I should like to bear tribute to the skilful manner in which the cave has now been lit up. I know of no other quite like it. Naturally any painted cave well illuminated loses its mystery. One no longer feels that one is really back in a time before agriculture or domestication of animals had been thought of and in the presence of the sorcerer leading his band into the cave depths there to perform the magic rites that all feel are necessary to insure their daily food supply. But, on the

other hand, at Lascaux to-day a detailed study of the paintings is easy as the lights have been so arranged that almost every drawing can be seen perfectly. They can be ascribed, as I think I have already indicated, with very fair certainty to the late Aurignacian folk while the art at Altamira in northern Spain is late Magdalenian. In these two caves can be seen the finest art of these two periods, and how different it is. In both, the treatment of the subjects is naturalistic although the animals at Altamira are perhaps more skilfully drawn and each has a more clearly noticeable individual character expressed by the artist. My own favourite bison on the ceiling is a friendly brute and would do me no harm if I turned my back on him. On the other hand, the one my late chief Dr. Obermaier always fancied I should not have trusted an inch. If I had turned my back on him he would have promptly gored me ! But marvellous as these paintings at Altamira are, they are static. Even the galloping pig does not really seem to get along. Quite different is the feeling at Lascaux where one is made positively giddy. The great oxen are pounding round the central dome of the cave, the delightful 'Shetland' ponies are trotting, the stags may even be swimming. All are alive and active. Truly the vigour and movement imparted to these drawings take one's breath away. It is too early yet to say how far this subtle difference in the art of these two caves represents more than a difference between the local artists who made them, or whether it has a wider cultural significance. But it is a field for study.

Another as yet unworked-out problem of this period, this time one for the specialist with a wide knowledge of its flint industries, is that of the multiple origin of the Aurignacian culture. Where did each of its three contributory streams originate and how did they arrive in western France ? Translated into terms of human peoples what *was* going on in Europe at that time ?

The prehistorian proper is fundamentally a quaternary geologist. The methods he employs to get his information are like those used by the geologist, and except now and then he is concerned rather with humanity and his products as fossils than as live entities. I do not mean to say that now and then he does not feel the pulse beating beneath the stone and see the flesh and blood on the skeleton, but (and shall I be shot at for this suggestion ?) should there ever be an ideal partition of collections, his interests are, or should be, more connected with South Kensington than with Bloomsbury ! When we come to the proto-historian the situation is rather different. Still the same scientific methods are employed both in the case of excavations and in other ways. But now we are able to think of the people as indeed our ancestors, simple in culture but like unto ourselves. The prehistorian deals with the hunters and food gatherers, the proto-historian starts with the food producers, and as we shall see in a moment this involves a profound change in the ideology of mankind. But it is a change that an intelligent child, specially a country one, can perfectly well appreciate. As I have just remarked, I always feel that the prehistorian deals with our forerunners, the proto-historian talks with our ancestors.

Food-producing is a characteristic of the neolithic civilisation and this civilisation was made possible by four new discoveries: those of agriculture, the domestication of animals, the making of pottery, and the new technique of grinding and polishing stone for tool-making. I am not proposing to discuss the neolithic civilisation in detail. That is outside the theme of this address. But there are one or two aspects which seem to me vitally important, which I have not found in the literature, and which are relevant to my main thesis because they do to some extent affect our outlook on the whole subject of mankind's development. The grinding and polishing of tools one can quickly dismiss. Its importance, of course, is in connection with carpentry : now for the first time a reasonably sharp edge can be formed on a tough fine-grained igneous rock which permits woodwork which was impossible to the carpenter armed only with brittle though sharp flint tools. Nor need I say much about pottery-making, although this discovery perhaps more than anything else in the remote past lead to cultural improvements in the home. Think what it must have meant when you could first take a drink from a decorated bowl with your supper instead of having to trot down to the stream afterwards like an animal. But the discoveries of agriculture and the domestication of animals, more particularly the former, did indeed revolutionise man's existence. From an early neolithic date we find the bones of two kinds of dogs : the one derived from the wolf—the herd dog, the other from the jackal—the house dog. There is some slight evidence for the existence of a domesticated dog in still earlier mesolithic times. Quite rightly has Kipling with his usual prescience termed the dog 'first friend.' We also have two kinds of sheep derived from two out of the four basal wild stocks, namely the urial and the mouflon. The former of these provided the first domestic sheep known in Europe. Again we have two varieties of cattle, the short-horned, the first to be seen in the west,

and a variety ultimately derived from the wild *Bos primigenius* that roamed about Europe during later pleistocene times, and that is depicted so frequently on cave walls by upper palaeolithic man. With the pig we more or less exhaust the list of the early domestic animals, and you will notice that the horse is absent, although he was roaming freely in the wild state throughout late pleistocene times. No one of the four main varieties of wild horse seems to have been domesticated, at any rate as far as Europe is concerned, until the Copper Age. But a herdsman may be a nomad and the thing which, if I may so put it, 'settled' neolithic man was his discovery of crop-growing. It is no good sowing the seed and then not waiting for the harvest—now to be stored for the winter in simple silos or large pottery vessels and ground to meal with grindstones and eaten perhaps by people wearing woven clothes and not only the skins of the hunters.

The old question is often asked, and I have seldom seen it satisfactorily answered, as to whether the neolithic civilisation developed in one area and then was diffused throughout much of the world, or whether it developed in more than one area, and if so whether these developments were contemporary or of very different dates. Personally I have always felt that the climatic circumstances leading to these new discoveries were a drying up of an area formerly fertile and the appearance of oasis conditions—later to be replaced by deserts. I ventured to suggest in a broadcast to the Far East an imaginary story of a small child sent to gather wild grain at a time when increased dryness was making life difficult and so stimulating the intelligence of human beings in the struggle to survive. On her way back, by the little dwindling stream she tripped and dropped her precious load. How she was whacked by her mother! But next year there was grain growing in the place. Father wondered why and thought about it and suddenly put two and two together. Again, if you read Kipling's *Mowgli Tales* you will remember the story of the peace rock. For 'peace rock' read 'oasis.' Here the animals collect together to drink, as indeed, too, must man. Such close contact may well have increased the number of tamed animals living near or in man's habitations. Taming is not domestication, but much taming may well have resulted in breeding in captivity, and once again man may have put two and two together and suddenly seen the possibilities. The Bushmen of South Africa who lived in an old Stone Age stage of existence never became herdsmen or farmers. Why? Because they had no need. With the teeming wild life around them existence was easy—at least before the last of them were driven by the white man into the desert and no longer had the spirit to survive. A few such areas, formerly fertile and now desert, do indeed exist. Central Asia is one and the Sahara another. Must we not, then, consider the origin of the neolithic civilisation to have been multiple? I feel so. Could it be shown that the drying up of the areas was the result of climate changes of a more or less universal character at the end of quaternary times when the glaciers and ice sheets of the Great Ice Age finally disappeared, we should be able to suggest that the neolithic cultures in the different areas were more or less contemporary. But more work needs doing in these difficult areas. We lack facts and information on which to theorise.

Now for a word about the changes in man's way of life and his outlook on it which necessarily followed these new discoveries. One of the first results was an increase in population. Among primitive peoples Malthus' law holds. The increased food supply and the fact that corn could be easily stored made it possible for more individuals to exist and so there came an immense increase in population, doubtless as a result of a decrease in infant mortality. Not all these extra individuals would have to be busy all their time in providing food for the community, life was less precarious, there would be more leisure and indeed there appears to have been some specialisation in function among the villagers. Again with the storage of food and keeping of flocks and herds man had to deal for the first time with valuable communal property: and this requires protection—not only from wild beasts. With the neolithic civilisation came the beginning of war. One is tempted to wonder whether Adam and Eve in the Garden in the bliss of ignorance or Palaeolithic man in his painted cave mouth were not better off than we twentieth-century know-alls of good and ill? However, I have not time to deal with all these matters in any detail. Besides, any one who thinks about it for a moment can fill in the outlines for himself. There is, however, one aspect which may not have struck you, but which is of great importance, and again is one which an older child can grasp and which will stimulate his thought. We have noted that with agriculture for the first time came real settled community life. Now we have the village which very soon, especially in the Near East with the slightly later discovery of metallurgy, developed into the small town. The problems facing man in his new community existence were not only material but psychological. People had to learn how to live together in close quarters. Those of us who had the good fortune or the ill luck to be

billeting officers in 1939 will know that neither the devil in hell nor Hitler in Germany could make two women agree over one frying pan in one kitchen. Again, life is not possible in a community if everyone freely steals, murders, commits adultery or tells lies under oath. And so throughout the Near East, as the villages developed into small towns, simple fundamental customs arose, necessary for the existence of any polity. But in an age before central government and policemen had appeared, how were these customs to be enforced? All we can say is that among the Mitanni of the upper Euphrates and again among the first Aryan invaders of northern India, before 1500 B.C., there arose the concept of a single God, Artha by name, who was interested in cosmic and social order—a sort of apotheosis of law—who was prepared to punish those who broke the local customs and habits that were by now being codified into laws. Compare Numbers, chapter xvi, the rebellion of Korah, Dathan and Abiram. It is significant that Akhenaten, the famous monotheist heretic king in Egypt whose hymn to the sun is so closely paralleled by our 104th Psalm, had for wife a Mitanni princess.

Clearly we have now reached the age of metal. A knowledge of metallurgy followed hard on the neolithic civilisation, but whereas the conditions which led to the former probably obtained in a number of different regions, and therefore the origin of the neolithic civilisation may have been multiple, the knowledge underlying the technical processes involved in smelting copper and tin ores is so complex that it would seem more probable that this science diffused from one single centre.

But I don't propose to carry this survey further. We are reaching out beyond the realms of prehistory well into those of the proto-historian, and anyway time is passing. I must, however, conclude by hoping you have seen in the course of this survey at least one or two of the reasons why I think the study of prehistory peculiarly important as a school subject. Let me say at once I am not thinking of the primary schools. More particularly in rural areas the large classes obtaining and their unselective character mean that the unfortunate school teacher can deal with little more than the elements. But in the case of senior modern and grammar schools the situation is different. Should prehistory ever become a recognised subject in whatever syllabus replaces the present one for either the school or the higher certificates, it would, of course, automatically appear in the curriculum of the latter. But I do feel that there are cogent reasons why it should be considered by headmasters and headmistresses of our senior moderns. (i) The subject is basal, it deals with beginnings—the beginnings of ourselves, our means of life, our thoughts, our religion. (ii) The methods employed for obtaining information, though easily understood, are scientific and logical, Many of them, too, can be demonstrated in practical work. Yet we are dealing with humanity and not with some impersonal scientific phenomena. (iii) The subject can be made enthralling, more particularly when cave life and cave art are being considered. (iv) While not necessarily the immediate concern of the school the knowledge imparted will result in not a few of the children becoming interested in their local archaeology, and this will stimulate their powers of observation and deduction and provide them with hobby interests for those leisure hours which with the shorter working day are the goal of modern legislation. (v) In the case of the rather older child the realisation of the great antiquity of man and the rise and fall of civilisations in the past, together with the growth and disappearance of populations as the result of forces entirely outside man's control, does put human history in a rather special and humility-producing perspective. Let me say at once that as a prehistorian I have found nothing to upset the fundamental truths of our Christian heritage. But I have found that the prehistorian views modern problems with a sense of proportion. All is on a wide canvas, he does not look through blinkers. I would rather some of the facts of prehistory, as indeed of geology, were understood by the youngsters before the 'certainty' of adolescence makes them assume that all the spiritual training of their youth, if they had any, was kibosh. I know the curriculum is very crowded, but I do plead that some of the items should be examined as to their importance. I was educated in private schools and at Eton. Throughout my boyish days I had to do algebra and Latin verses. I have not found those subjects of the slightest use to me in after life. The mental training given by such subjects could equally have been given me by a study of geology, which would have made every railway journey I took a far greater fascination. We are living in a time of flux. Many of our institutions are being stirred up. Before these settle down again let us consider whether this new and potent subject would not be more valuable than some which have their acknowledged places to-day. Of course, there is the problem of teachers. But this does not worry me. The subject is so fascinating that any school teacher deciding to 'mug it up' will find himself caught up, as it were, and reading it as he would his favourite novel.

SECTION I.—PHYSIOLOGY

THE CELL

ADDRESS BY

PROF. R. A. PETERS, M.C., M.D., F.R.S.

PRESIDENT OF THE SECTION

THE President of Section I of this Association can choose either to review some speciality in which he may be particularly interested, or he may attempt a more general summary of some aspect of the field which he represents; it is this latter which I feel to be the most useful thing for me to do to-day. As my own interests have lain towards the more biochemical sides of Physiology, I propose to discuss mainly the present state of biochemical knowledge of the organisation of enzymes in the cell. Clearly the subject is too large to cover in more than outline, and the development of present work too extensive for one individual even to have the presumption to speak. This address therefore will form the introduction to a discussion of some aspects of knowledge of the cell as a whole, and it is hoped that even this limited presentation may help those not so well acquainted with the modern position to appreciate what is in progress.

My primary object to-day is purely scientific; at the same time it should be realised that accurate understanding of the chemistry of the living substance is the most powerful approach to rational control. It is significant that during the last years for the first time the rational biochemical approach has given therapeutic results; in my laboratory during the war it led to the development of the arsenical antidote British anti-lewisite and has led elsewhere to important synthetic curare-like substances, such as are being used in anæsthesia.

There have been several notable addresses by past Presidents of this section related to to-day's discussion. That by Gowland Hopkins in 1913 may be said to have laid the foundation of the modern way of thought, to have abolished finally the inhibitory view of the large protoplasmic molecule which set all accurate chemical work out of reach, and so to have set us on the exploration of dynamic biochemistry. In the light of his work with Fletcher upon the conditions for the formation of lactic acid in muscle, Hopkins showed how a study of the behaviour of small molecules might lead to facts important for life. In 1930 we had an important appreciation of knowledge of synthetic activities of the cell by H. S. Raper. A more philosophical statement upon 'Form and function in living matter' was made by Leathes in 1926, in which review he treated of chemical structure, though in somewhat too pessimistic a vein, as I think, in regard to the proteins.

The development of our knowledge of the constituents of the living cell and of their interrelations has made astonishing progress in the last 20 years. We have only to choose as an example the vitamins to see what has been happening. These substances are present in minute amounts in the tissues and had not even been isolated in a pure state some few years ago. Now 17 are known and many have been synthesised by the organic chemists; they may even be regarded as 'fine chemicals.' It is also known that several members of the vitamin B complex form part of enzymes or enzyme systems in the cell. It is almost banal to mention the improved techniques which have become available in the last years as the result of advances made and of interest shown by chemists and physicists; we have for instance X-ray crystallography of proteins, ultra-violet and infra-red spectroscopy, the ultra-centrifuge, the electron microscope and the development of work with tracer elements; also due to the labours of biochemists especially we have much improved methods of estimating trace substances, micro-techniques, including chromatography and the skilful use of filter paper; it has even become possible to work out techniques for the analysis of the spatial position of enzymes inside the cell. All these advances have proved rightly very exciting and have led to important new observations. We have to ask ourselves quite objectively how much more we now know about cell organisation as compared with information obtained by cytologists 25 years ago. Nothing should divert us from our final aim of describing and understanding the living cell itself in biochemical terms. If to-day's discussion serves to focus attention upon some of the main

points it may help to define them. I would emphasise again, the subject is so large that it could do no more.

On the last occasion when this Association visited Newcastle in 1916, the distinguished pharmacologist A. R. Cushny gave a Presidential address to this Section which I find to be of the greatest interest. It was entitled ' The Analysis of Living Matter through its Reaction to Poisons.' In this he considered how far a knowledge of the detailed action of drugs upon cells could be used to subserve the wider function of teaching us something about the active groups in living matter, a conception as it were of biochemistry in reverse. In some sense this is in line with studies which have occupied our school in Oxford for some years, and which have had a dual origin from work upon the problem of therapy of war gases and upon vitamins of the B complex. I shall use some of this work to introduce my subject to-day ; my main aim is to draw your attention to the present state of knowledge upon organised enzyme systems, but I shall conclude with a brief discussion of more structural points related to cells. Also there will be a subsidiary theme forming an accompaniment in the bass (to use a musical phrase), dealing with the very real question how far modern biochemical schemes can be applied in the physiology of the living cell.

The Pharmacological Action of Iodoacetic and Fluoroacetic Acids

Entering upon my main theme, I will start by drawing your attention to two very simple, but very poisonous substances ; these are iodoacetic ($CH_2I.COOH$) and fluoroacetic ($CH_2F.COOH$) acids. Both will kill mammals when injected, the latter is more poisonous weight for weight than the former ; in the body at *p*H 7·0 they will exist mainly as iodoacetate and fluoroacetate. The question is, how do these small molecular substances kill animals ? Though both are toxic and though they are rather similar apparently in chemical structure, their action in biochemical terms is quite different, and it is based upon an extraordinary difference in their chemical behaviour. Though they are both halogen substituted acetic acids, the iodine atom of iodoacetic acid is easily capable of taking part in a substitution reaction at neutral *p*H. On the other hand, fluoroacetic acid was shown by Swarts (of Ghent in Belgium) who synthesised it, to have a very stable C-F group, which even withstood the action of concentrated sulphuric acid. Iodoacetic acid is not found in Nature as far as I know ; but it has been proved in recent years by Marais (1944) that fluoroacetic acid is the toxic principle of the poisonous plant in South Africa known as ' Gifblaar,' which has been responsible for the deaths of sheep and cattle. The true botanical name of this plant is *Dichapetalum cymosum*. There have been several studies of this plant over many years, and curious things have been found out about it which are only incidental to my present remarks. For instance the leaves are more toxic in the spring and autumn when the newly formed ones are growing. The fluoroacetate present is of course tasteless and found in an extract of the leaves of the plant. It is possible that other varieties of the species of Chailletia found in Guinea and Sierra Leone have even been constituents of poisons known to local doctors. One of these is known as ' rats-bane ' and the powdered fruit has been used as a rat poison.

Iodoacetic acid and SH groups.

Turning first to iodoacetic acid—this substance has played an important part in the development of some aspects of enzyme biochemistry and especially of muscle biochemistry, because of its property of combining with SH groups according to the following equation :—

$$GSH + CH_2I.COONa \longrightarrow GS\,CH_2.COONa + HI$$

It is therefore not the iodine but the capacity for blocking the SH group which really matters. In accounting for the toxic action, it may be said that there are now grounds for the belief that several enzymes require a free SH group in their protein component for the manifestation of their catalytic activity ; if this is blocked, stages in intermediary metabolism which depend upon the action of these enzymes will not proceed and the body will be unable to get the energy it requires. Not all SH groups in enzymes are equally attacked by iodoacetate ; some, for reasons still not quite clear, are relatively insensitive to this reagent. In the address to which I have referred, Hopkins mentioned experiments upon the conditions for the formation in muscular tissue of the simple molecule, lactic acid. Indeed this substance formed a prominent feature of theories of muscular contraction until the end of the second decade of this century. At this time it was discovered by Lundsgaard that when animals were killed by the injection of iodoacetate or when this substance was added to enzymes prepared from isolated muscle tissue in vitro, the appearance of lactic acid was completely stopped ; a muscle contracting in vitro contracted without lactic acid formation ; hence ideas on muscular motion had to be altered because contraction could

proceed without lactic acid formation and the appearance of this acid was not directly related to the contractile process. I have dwelt upon this because it was a most fundamental observation. It showed that some intermediate steps in the transformation of sugar to lactic acid must be blocked. Biochemists have now worked out in some detail the series of phospho ester transformations which intervene in the degradation of sugar, starting from initial schemes of Embden and Meyerhof. The diagram (Fig. 1) gives the results of these labours and it should be

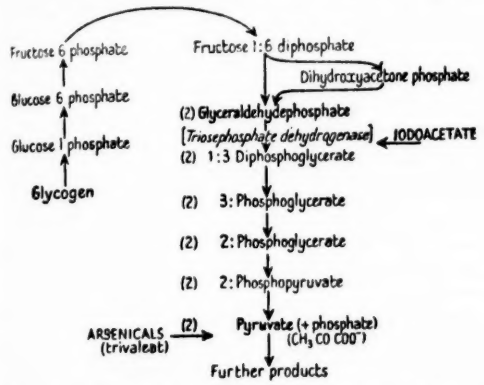


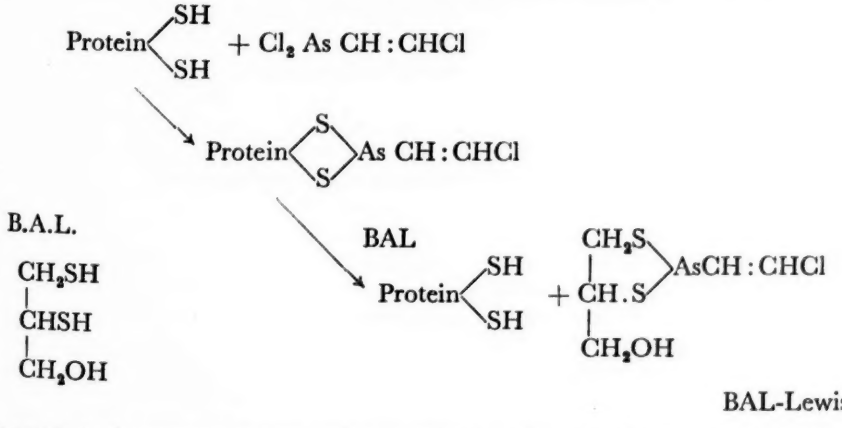
FIG. 1.—Course of reactions followed during the degradation of glycogen to the 3 carbon stage of pyruvic acid ($CH_3.CO.COOH$), showing points where iodoacetate and trivalent arsenicals interfere. The stages are catalysed by specific enzymes.

realised that each stage is catalysed by a specific catalyst. It has been found that the triosephosphate dehydrogenase responsible for the stage indicated is specially sensitive in vitro to the action of iodoacetate, and it is at this stage that we should consider the block due to iodoacetate first takes place. If higher concentrations of iodoacetate are used, enzymes which are less sensitive to the action of the poison may also become involved. We therefore see from this biochemical work that we have a rational explanation for the toxic action, which is based upon the idea that interference with the action of an enzyme in the cell may prevent the normal production of energy necessary for muscular work. In this case the biochemical analysis of the pharmacological effect has not been pushed as far as the proof that the triosephosphate accumulates in vivo.

Other poisons and SH groups.

In the case of other poisons acting upon SH groups, the analysis has been much further; I refer to investigations in my laboratory dealing with interference at another stage in the metabolism of carbohydrate, the pyruvate stage. The oxidation of pyruvate is carried out by a battery of enzymes, known as the 'pyruvate oxidase system.' Though trivalent arsenical compounds attack other enzymes in relatively high concentration, the pyruvate oxidase system is wrecked by mere traces of an arsenical such as lewisite ($CH.Cl:CH.AsCl_2$); this is an attack upon a specially arranged dithiol grouping, and the action of the arsenical can be reversed by the antidote known as British anti-lewisite (Peters, Stocken and Thompson) as shown in the equation below (Fig. 2). The results have proved most important in medicine both in mercurial and arsenical poisoning, as well as in chemical warfare. In vitamin B_1 deficiency we have an instance of a biochemical lesion induced in this same pyruvate oxidase system; here it is not due to an attack upon an SH grouping, but to the loss of the essential coenzyme containing vitamin B_1, viz., Thiamin-pyrophosphate. In all these cases we have strong grounds for thinking that the

FIG. 2.—Mode of action of B.A.L. (dimercaptopropanol).



The protein in the pyruvate oxidase system contains 2 contiguous SH groups which combine with trivalent arsenic (lewisite) to form a ring; this is stable, but less so than the BAL-Lewisite compounds; hence the As is transferred to the antidote.

interference with the cell is initially due to the elimination of one of its enzymes ; that is to say it takes place inside the cell rather than upon some component in the external surface of the cell.

Fluoroacetic acid.

I have already said that in the case of fluoroacetate the action is different, owing to the stability of the C-F bond. The toxicity of this has been worked out by pharmacologists and is summarised in Table I. You will see that it is very poisonous indeed to an animal like the dog. Most surprisingly, the African clawed toad (*Xenopus laevis*) does not succumb to a dose 10,000 times greater than the dose which will kill a dog. There is a macabre connection here between the absence of effect on the toad and toxicity to the higher animal, especially when considered with the suggestions in the literature to which I have already referred that another species of the genus found in the Sierra Leone region has been well known to the local doctor as a useful poison for enemies. The description of deaths of domestic animals from poisoning by the latter leaves a strong impression that the active principle is here also a fluoro compound of this type. Sodium fluoroacetate produces predominantly two classes of effects, those upon the central nervous system and those upon the heart ; in the heart it can lead to a complete heart block. A prominent feature of the action is that there is a latent period during which nothing at all may happen. Tests so far carried out have not revealed that there is any enzyme in tissues liberating inorganic fluoride from fluoroacetate ; hence the toxicity is not due to fluoride. Much work in England and in the United States has not revealed any isolated enzyme which is poisoned by fluoroacetate in vitro ; in this it is again very different from the compound diisopropylfluorophosphonate which was made during the war and which is a specific inhibitor of choline esterase. In the absence of a demonstrated effect upon an enzyme, it might be thought that the toxic effect was exerted upon the surface of cells ; but such a view does not seem reasonable with so simple and so water-soluble a substance.

TABLE I.—*Fluoroacetate poisoning. Responses of various mammals.*

	LD_{50} mgm./kgm. of methyl fluoroacetate	Cardiac response	CNS response
GROUP I.			
Rabbit	0·20–0·25	Ventricular fibrillation	None
Goat	0·60	,, ,,	,,
Horse	0·50–1·75	,, ,,	,,
Spider monkey	14·0	,, ,,	,,
GROUP II.			
Cat	0·5	Slight	Marked
Pig	0·4	Response of both heart and brain about equal	
Rhesus monkey	4·0	Ventricular fibrillation	Very slight
GROUP III.			
Dog	0·06	None	Marked
Guinea pig	0·35	,,	,,
GROUP IV.			
Rat	5·0	Atypical	
Hamster	2·5 –5·0	,,	

Chenoweth and Gilman (1946).

In searching for other explanations, Bartlett and Barron (1947) discovered that though isolated enzymes are not poisoned in vitro, more organised tissue preparations such as the 'Warburg' tissue slice may show decreases in oxygen uptake (respiration) induced by fluoroacetate with accumulation of acetate. In kidney preparations made in a special way, we found (Liébecq and Peters) last year that *in vitro* the addition of fluoroacetate causes an accumulation of citrate together with a decreased oxygen uptake; in the ordinary way such kidney preparations will oxidise citric acid well. A further important step has been taken very recently; after injection of this poison into animals such

as the rat, a relatively enormous accumulation of citrate is found in many of the body tissues and especially in heart and kidney, so that the same phenomenon is found *in vivo* as occurs *in vitro*, (Buffa and Peters, Table II). We are faced at once with the

TABLE II.—*Effect of fluoroacetate upon accumulation of citrate in the rat.*

Tissue	Control	Poisoned	Increase
	Citric acid μg./g. wet tissue		
Blood	3	50	16 times
Liver	0·8	31	40 „
Brain	21	166	8 „
Heart	25	677	26 „
Kidney	14	1036	56 „

The animals were injected by intraperitoneal route with NaFlacetate (5 mg./kg.) in 0·9 per cent. NaCl and killed 60 min. after injection ; controls similarly injected with 0·9 per cent. NaCl.

P. Buffa and R. A. Peters, 1949.

curious dilemma that fluoroacetate is not toxic to any known enzyme, and yet its introduction into the body, as well as its presentation *in vitro* to the kidney preparation, causes the accumulation of citrate—an effect which it is hard to think is not due to action upon some enzyme. Citric acid is of course a well-known constituent of fruits ; for long it has been known also that it was formed in the mammalian body ; and more recently it has been found in bone and skin (by Dickens). To obtain a reasonable interpretation of these curious facts, we have to turn to some very modern biochemical conceptions.

Cyclical biochemical schemes.

For many years there have been theories that some of the chemical reactions in the body took place in cycles. Early cyclical schemes were presented by Meyerhof for muscle and by Keilin for oxygen uptake. In connection with the degradation of pyruvic acid (of which I have spoken earlier), an ingenious scheme was suggested by Toennissen and Brinkman (in 1930), involving the idea of condensation of two pyruvic acid molecules to form a hypothetical 6-carbon compound, which was successively transformed through succinic, fumaric and oxaloacetic acids back to pyruvic acid with the loss of one molecule of pyruvic acid during the transformation. Further work by Szent Gyorgyi and colleagues (1935) emphasised the importance of the three dicarboxylic acids in muscle ; also Knoop and Martius (1936) demonstrated the presence of enzymes in tissues concerned with transformation of ' the citric acid ' series of acids. The final synthesis of these ideas has been made by H. A. Krebs in the scheme known as the ' tricarboxylic acid cycle,' which is shown in a modern dress in Fig. 3. Briefly there is believed to be

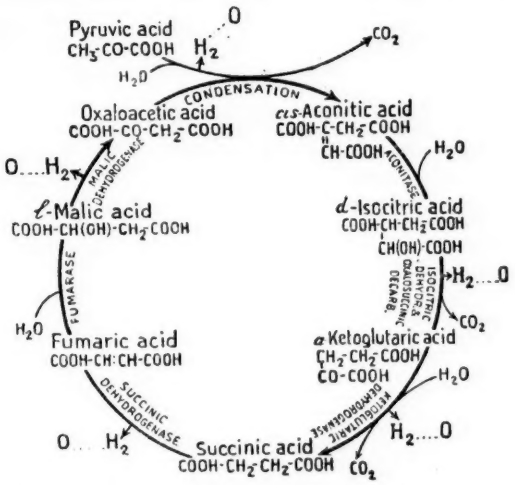


FIG. 3.—The diagram represents the reactions of the tricarboxylic acid cycle, in which a 2 C fragment formed from pyruvate is condensed with oxaloacetate to form a 6 C tricarboxylic acid (3 COOH groups). This is then degraded to acids with 4 carbon atoms. CO_2 is split off at various points of the cycle, and also H_2 which is taken up by the cytochrome system. During one round of the cycle, one molecule of pyruvate is degraded to CO_2 and H_2O with uptake of 5 atoms of oxygen.

(After T. Mann.)

present in many living tissues a complex of enzymes, which in a very real sense appear to form some part of the organised central machinery of the cell dealing with the final stages of carbohydrate and fat metabolism. The essential idea of this is that a molecule such as that of acetate or of pyruvate, from which acetate (or a 2-carbon intermediate) may be formed by decarboxylation, is oxidised by traversing a cycle of reactions, involving at one stage a complicated synthesis of an acid in the citric series. Reference to the diagram previously given will show that pyruvate is a prominent stage in the breakdown of sugar.

Considering the oxidation of pyruvate via this ' tricarboxylic ' cycle, the initial step is the formation of an unknown 2-carbon fragment from pyruvate by the action of the first component of the pyruvate oxidase system. This C_2 fragment then reacts with oxaloacetate to form a 6-carbon acid with 3-COOH groups in the ' citric ' series. The more recent idea is that the actual acid first formed may be cis-aconitate ; accordingly citric acid itself would be in equilibrium with this, though not on the main path of

metabolism through the action of the enzyme 'aconitase.' Though there are details still to clear up, the essential point is the condensation followed by the successive formation of the 5-carbon acid *a*-ketoglutarate, then of the 4-carbon acid succinate, which is eventually transformed to oxaloacetate; thence the whole cycle can start again. With one turn of the cycle, there is a disappearance of one molecule of pyruvate with the uptake of five atoms of oxygen and the evolution of three molecules of CO_2.

Before applying this scheme to the case of fluoroacetate which we have been considering, it should be said that the C_2 fragment shown in Fig. 3 to be formed from pyruvate by oxidative decarboxylation, may also be formed from acetate by a special enzyme; when this happens the acetate can also enter the cycle and be oxidised after condensation with oxaloacetate.

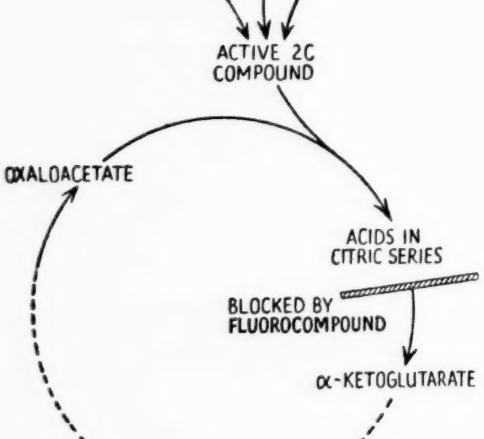


FIG. 4.—The diagram represents the course of reactions in the "jamming" hypothesis of fluoroacetate action, as suggested by Liébecq and Peters (1948) and extended by Buffa and Peters (1949). It incorporates the idea of Bartlett and Barron (1947) that fluoroacetate competes with acetate in metabolism and is consistent with those of Saunders (1947).

These biochemical conceptions now bring us to a reasonable new hypothesis for the reconciliation of the contradictions to which I have referred. Upon the basis of their work Bartlett and Barron had suggested the hypothesis that fluoroacetate would compete with acetic acid and prevent its oxidation via the tricarboxylic cycle. Liébecq, Buffa and I now suggest that this competition is not due to a block at this stage, but occurs because the molecules of fluoroacetate can be activated by the same enzyme as acetate and then can enter the cycle. The foreign molecule so formed cannot be handled by one of the enzymes at the tricarboxylic acid stage and the foreign molecule 'jams' the 'cycle,' leading to the gradual accumulation of citrate formed in metabolism from carbohydrate or fat (Fig. 4). By this hypothesis, we can reconcile some of the apparent discrepancies, and get an insight into the biochemical happenings preceding the toxic effects, what we call the 'biochemical lesion.' On this view the fluoro-compound is poisonous because it is so like a normal metabolite that the tissue enzyme concerned cannot distinguish between it and normal acetate. Why the accumulation of citrate is toxic we do not yet know. If our ideas stand the test of further work this is rather a new way in which enzymes inside the living cell can be indirectly attacked. It is, as it were, another example of a protective synthesis gone wrong.

I have compared the way in which these two rather simple substances poison the body in order to indicate how far our knowledge of the organisation of enzymes in cells has now advanced and how much more can be explained in pharmacology than was possible a few years ago. There is now much evidence that this central 'enzymic engine' in the metabolism of the cell is the final stage through which pass the metabolism of fat as well as of some elements of the protein metabolism.

The Bridge between Biochemical Schemes and Physiology

Many (in fact most) details of these biochemical integrations have had perforce to be worked out by biochemists after very brutal treatment of the tissues. In Sherrington's words, 'To many biologists this (grinding into unrecognisable fragments) seemed a Philistine procedure. But it was an audacity which reaped a great harvest of new knowledge. Their mush of unrecognisably disintegrated cells yielded to chemical analysis secret after secret of cell life.' It is the method by which biochemical results have to be reached which has made it most important to obtain bridges between the test-tube experiments and the living body itself. I know that many biologists are disinclined to take the schemes of the biochemist seriously; in fact we encountered this feeling during the preliminary work which led to the development of British anti-lewisite. I want now to suggest even with an emphasis that may be considered too bold, that in the case of the metabolite pyruvic acid we have had this bridge for a long time in the vitamin B_1 work, and that now in this citric acid formation in vivo we have another missing link. Pyruvic acid does appear (as pyruvate) in the blood in vitamin B_1 deficiency as also in arsenic

poisoning—both conditions which wreck the pyruvate oxidase system *in vitro*; fluoroacetate poisoning induces accumulation of citrate *in vivo* as well as *in vitro*. Hence I would suggest that the onus is no longer upon us if this biochemistry is not taken seriously.

I have touched only briefly upon respiration and have no time to do more than state that here too there is now a well recognised scheme for the enzymatic reactions (Keilin). It has been a great change for one like myself who learnt his physiology 40 years ago to be forced to realise that the oxygen going into the body does not form part of the CO_2 which comes out, and that its function is mainly to form water by changing the valency of a porphyrin-iron compound. It has been revolutionary also to realise that CO_2 may be used for synthesis in the animal as well as in the plant.

Some General Considerations

Before turning to more general considerations, there are some aspects of life which are so fundamental that they require a mention in any statement about it even at the risk of the banal. Firstly in the higher organism there is the dominance of water; even brain tissue is about 80 per cent. water. Then there are the limitations of temperature; in the mammal, an organism with a temperature regulated about 37° C., a rise of even 5° C. up to 42° C. may cause the tissue cells to show abnormal permeability, as was shown by Kellaway and Rawlinson. There are also the limitations of oxygen; Fisher and Parsons have found recently that even a 10-second interruption of O_2 is enough to alter enzymic reactions in the intestinal mucosa. Again we must never forget the necessary salt content of the internal environment or that the bulk hydrogen ion concentration of the cell is regulated very near to the neutral point. Any chemistry in operation must be a chemistry applied between these limits.[1]

Assuming that these organised complexes of enzymes of which I have spoken exist in the cell, where do we find them, because it is certainly part of our task to fit them into their proper environment? In the past it has seemed to be reasonable to consider that the nucleus is the site of directive influences in the cell. We also used to believe that all the nucleoproteins were in the nucleus. Now our views have changed with the finding that some nucleoproteins, predominantly the ribonucleoproteins, are also in the cytoplasm (Casperson, Brachet, Bensley). This whole subject will be reviewed by Prof. Davidson; it suffices here to say that studies of the cell fragments used in biochemical work by the methods of the high speed centrifuge show that fractured cells can be separated into several distinct layers, variously called 'granules' or microsomes; among these are to be found some of the enzymes mentioned, many of which are therefore in the cytoplasm. I suppose that in the last limit it is not easy to be certain that all these 'granules' are present as such in the cell initially, but they must represent some special associations originally there. One whole Danish school, that of Linderstrøm-Lang, is devoted to the accurate study by ultra-micro methods of the location of enzymes in cells, work of the most fundamental importance.

Considering how the enzymes may be ensconced in the 'granule,' the simplest idea would be that they were merely enclosed haphazard in a kind of sac, as the contents of the nucleus might be thought to be enclosed by the nuclear membrane. This idea will carry us some distance in explanation of some of our findings. For instance, it has been known for some time that cytolytic agents could wreck organised enzyme reactions which depend upon surfaces; there is the familiar example of the stopping of glycolysis in the blood corpuscle by cytolysis. In our work with brain we used to think that the difference between more finely ground brain homogenates and the minced brain or 'brei' was due to the presence of the intact nuclei in the brei; it would be equally explained by the view that some of these 'granules' from the cytoplasm are fractured, the membrane enclosing the sac of enzymes so destroyed and the contents much diluted by liberation.

I do not myself think that relatively unorganised inclusions of enzymes in the 'granules' can explain the phenomena found in the pyruvate oxidase system; and I find that Keilin and his colleagues also think that there is a structural element in the relation of the cytochromes and the succinodehydrogenase system. In my view these enzymes are structurally related and disturbances induced by biochemical methods may lead to a disorientation of enzymes originally distributed spatially in relation to one another. It is difficult to get cogent evidence upon this point, but I should like to direct your attention to one quite simple experiment which we have made. The dark green chromoprotein found in the lobster egg has been termed ovoverdin; it is a compound of astaxanthin, the red lipochrome, and of a protein. Boiling it, of course, leads to a separation of the two components, giving the familiar colour of the

[1] Chemical structure in biology will also be subject to the rapid dynamical exchange or flux envisaged by Schoenheimer and others as the result of work with isotopic and radio-active elements.

boiled lobster. The same change may, however, be induced very simply indeed at room temperature by the addition of minimal amounts of a fatty acid soap such as sodium laurate (C_{12}). The addition of this to a solution of ovoverdin quickly changes the colour to red. If calcium salts are now added, there is an immediate reversion to the green colour again, due to the withdrawal of the fatty acid by precipitation of this by the calcium salt (Peters and Wakelin). In this experiment the sodium laurate molecules have penetrated and separated the two parts of the chromoprotein ; whence we see how important factors of surface can be even down to a molecular level. We also see the kind of problems the cell is faced with in protecting itself against substances that may appear in the course of its metabolism, which might well wreck the machinery itself.

Following from the initial investigations which proved the orientation of molecules in surfaces some 30 years ago (I refer to those of Hardy, Langmuir and Harkins and of Adam), we now have an immense amount of exact information available upon the composition of surfaces, which Prof. Frazer will be summarising later.

The Biochemical Organisation of the Cell

Before finishing this address, I feel that it would lack courage upon my part if I did not touch upon a central problem of the cell, namely that of its organisation as a whole in biochemical terms. In doing so, it is difficult to avoid the charge of being perhaps too speculative to be useful. I am glad to say that we shall be able to hear Dr. Barer speaking upon these more biophysical and structural aspects from the point of view of new methods of microscopy and later Dr. Swann from that of sea-urchin eggs. I have already indicated that there is organisation of individual enzymes into enzyme systems ; that these enzyme systems themselves may be isolated in different cell inclusions and that surfaces of separation in the cell may have individual organised structure. These facts explain much ; are we to think that they are in themselves sufficient to explain what we know about the varied cell activities ? As I see it, there are two outstanding things which must be explained by a hypothesis of cell organisation.

(i) The first is that the cell at one given moment must be carrying out various chemical reactions which individually require quite different physicochemical environments, as judged from the behaviour of such reactions in bulk phases in vitro ; this is really the ' microkitchen ' argument of Hofmeister. I can illustrate it briefly ; for instance, it is known that the optimum *p*H for many enzymatic reactions in vitro is often far removed from that of the neutral point, *p*H 7·0, the bulk *p*H of the cell ; does this necessarily mean that such enzymes are always working far from their *p*H optima ? Again, some enzymes require the presence of an SH group for their activity, whereas other active proteins such as insulin and trypsin are no longer active when the SS bond which they contain is reduced to .SH HS.

(ii) A second important point is that the cell must be able to react as a whole to a stimulus applied at one point of its surface. It was calculated by the pharmacologist A. J. Clark that the quantity of acetyl choline required to produce an effect upon the heart cell would only cover 1/6000 of its surface ; that is to say, a drug can influence the whole of a cell when presented to a relatively small part of the cell surface.

To both these postulates there is to be added a third consideration ; the effects in question are proceeding in what may be termed a jelly : in essence it is a fluid medium, capable even of showing visible stratification when centrifuged ; even the protein constituents themselves are now believed to be in a rapid state of anabolism and katabolism.

Let us consider the first or ' microkitchen ' point and, as an example, take this in relation to the apparent contradiction that the *p*H of the cell is not that for the enzyme *p*H optima. We might say that each ' granule ' had a different *p*H and that each enzyme was enclosed in a sac, where the *p*H was optimal. This would require a very large number of such separate bags, and it is hard to see how diffusion of intermediate products from one bag to another would not limit the rate of reactions in a way that is not found. There is evidence however in regard to surfaces which is easier to understand ; it has been proved (Peters, 1931) that the apparent *p*K (dissociation constant) for a fatty acid or amine ionising in an interface is shifted 3·0 *p*H units to the alkaline or acid side respectively, as compared with the *p*K for acids or amines found in a bulk phase. Hence, so far as optimum *p*H is concerned, if the site of enzyme actions was confined to some internal surfaces, then conditions for the apparent *p*K could be arranged to cope with the difficulties which I have stated. We do not yet know in actual size how small such a piece of surface could be without upsetting this relation ; but we do know some curious facts which make it unlikely that the ordinary statistical equations apply. It is not realised generally that we can just see with the high power of the microscope a volume of water of 550 million molecules,—this would be a

cube with a side of $0{\cdot}25\mu$ ($2{\cdot}5 \times 10^{-4}$ cm.); upon our ordinary equations such a volume only contains at the neutral point and cell pH, on average, one H ion and hence one dissociated water molecule. Any entities, which are below this in size, 'granules,' or 'virus' and the like must have numbers of water molecules below the statistical average for the dissociation of one molecule. These would be really under control of the structure of the surface rather than bulk phases; surface structure if we include internal surfaces is dominant in life. Since I first pointed this out some 20 years ago and indicated some of its chemical consequences I have seen no allusion to it; but I am most interested to find that the mathematical physicist Schrödinger has now stated that the microstructure in cells must dominate their chemistry. Briefly he says that in life 'order comes from "order," and not from "disorder" as in chemistry and physics.' I am convinced that we have not yet realised the exciting realm of new chemistry to which the cell is pointing. A most important case is that of Sonneborn (1947); one particle of paramecin, the killer substance for paramoecia, is enough to influence the behaviour of subsequent organisms produced by division. Here it is obvious, as he points out, that mass action laws cannot apply. Of course, if we are once convinced that the structure of the cell eludes the usual statistical laws, then the way is open for understanding how the microkitchens can work. At the same time, we come soon to the idea that this microheterogeneity of internal surfaces can extend down to very fine limits, whether we regard these as 'aperiodic crystals' with Schrödinger or not. Still, I do not think we need more than the idea of segregation by varied surface structure to explain the 'microkitchen' phenomena, combined of course with specificity of enzyme action.

When we come to my second postulate, that the cell reacts as a whole to a small surface stimulus, in numbers of molecules, I cannot picture this upon a basis of 'bags' of enzymes floating in a chaotic protoplasm. To me, it does not become intelligible until I take the view that the cell is organised as a whole. The benzene ring reacts as a whole and this organisation enables us to interpret things about the carbon atoms of which it is composed which are otherwise unintelligible; so I believe it to be with the living cell. There is naturally some hesitation upon the part of many biochemists in accepting this because it appears to put experiments beyond reach, even to be a partial return to the dreaded and utterly sterile theories of the giant protoplasmic molecule. I think that this is a wrong point of view. The protein molecule is for instance a large and well ordered organisation; we have to study it first from the standpoint of its constituent amino acids and peptides and from this we hope and have much encouragement from experiments in Chibnall's laboratory that the whole molecule will be known. Equally we must know all about the constituent nucleoproteins and individual enzymes in the cells, before we can hope to see their interrelated structure; but it is a help to realise that there is this goal of structure before us. I would suggest that it really is important whether we do our work on the assumption that the cell is a mere dynamic equilibrium of colloids, or whether we think it may be structurally organised as a whole.

As a matter of fact, several biologists over the years have felt that there must be present in cells a sub-microscopic organisation, and rather recently some consider it to be a proven fact, which is probably going too far.[1] I was among these, though I am not certain that my position has been quite understood. About 20 years ago, I indicated the notion that there was present within cells a kind of mosaical three-dimensional structure, which need not be many molecules thick, but which was capable of acting integratively like some nervous system to give a co-ordination somewhat like that envisaged by Sherrington in his postulates of the integrative action of the nervous system for the whole body; I have elaborated this somewhat since then. There would be contact between receptors on the cell surface and this mosaic-like structure, which was given the better name 'cyto or cell skeleton' by Joseph Needham; this term I would adopt with the reservation that I do not mean a structure which is inert, like the ribs which encase the thorax. The 'cytoskeletal' hypothesis includes of course the assumption that there are fibres in cells maintaining the gel-like structure, to which others such as Meyer have alluded; but this is only part of my hypothesis. Nowadays we have information about the behaviour of proteins as fibres which makes it less hazardous to picture an organised cytoskeleton especially now that it is believed that nucleoprotein also exists in the cytoplasm. The difficulty of reconciling fluidity of structure with some rigidity has for long been met by the suggestion that we have a manifestation of thixotropy like that seen in a Bentonite gel. If it is felt that these views are unjustifiably speculative, I can perhaps quote the remarks of Leathes, who in the address to which I have referred said :—

'Such an analysis of the chemical meaning of material life viewed in the light of scientific

[1] Much information on this will be found in works by Fáure Fremiet, Frey-Wyssling and R. Chambers.

fact has to be largely an exercise of the imagination, but it may present itself as an intellectual necessity.'

I have avoided somewhat purposely the all important activities of cell division and of organisers because of their great complexity ; they are well and monumentally reviewed by Joseph Needham (1942) in his book on Biochemistry and Morphogenesis ; it seems to me to be a better approach to try to understand first the non-dividing cell. Nevertheless there is one problem related to this to which I should like to refer. It is fashionable in some quarters to consider the gene as an enzyme. I submit that it is still an open question whether the enzymes themselves here are to be regarded as the primary structure or whether there is a protein ground work to which the enzymes are attached, which is the true characteristic of the individual cell and which is likely to be based upon nucleoproteins. Recent work upon paramecin and plasmagenes is particularly pertinent here. The individuality of the cell has also seemed to me to extend beyond the enzyme, the catalyst tool of the cell. For instance, the failure of attempts to use skin grafts from even a near relative during the war for the repair of skin burns appears to be only explicable by taking the view that there is some protein constituent in the skin graft which is different for every individual in the world to-day. To understand this in final biochemical terms will surely be a task of much magnitude ; but though we should not minimise these difficulties, I do not think we should be depressed. The progress that has been made even towards the understanding of the action of quite simple molecules upon the body such as those of the halogen acids gives much hope for the future. Unlike the position which Hopkins faced 37 years ago, we now have the active help of the pure chemists and pure physicists at every point. In fact I should not be surprised if some of our ' purer ' friends secretly thought that biochemists had become superfluous ! If there be such, I suggest that they ask themselves how often it has happened that the biochemist has initiated a new technique ; they will not have to search far. The help of chemistry and physics will continue to be increasingly important so long as biological complexity is not overlooked. Yet I doubt whether we shall ever be able to dispense with some folk whose function is to undertake the special thought and experiments needed to get together and sift what is being discovered elsewhere. It will always be the peculiar task of the biologists, the biochemists, biophysicists and physiologists to be primarily interested in the cell itself and to choose the experimental simplifications likely to be valuable in the investigation of life.

Again in this address we can apprehend the unity of nature ; superficially indeed there would have appeared to be little connection between a mysterious plant poison from Africa and biochemical schemes for citric acid formation in animals. I have tried to show how an attempt to analyse the pharmacological action upon tissue cells of two very simple molecular substances leads to an application of recent biochemical research upon organised enzyme systems, indicating a central enzymic mechanism in cells. The further attempt to see this knowledge in relation to the living cell itself leads to a vision of problems of further complexity, but of absorbing interest, to which active work by cytologists, biophysicists and others is contributing. Such information can only be a start, but we may hope the future will show it to be in the right direction. I can but hand on the torch lit by Cushny in Newcastle to some future President of this Section.

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Note.—Only some guiding references are given. Since the address was written it has been learnt that independently about the same time C. Martins proposed a similar hypothesis for fluoroacetate action based upon experiments in vitro.

SECTION J.—PSYCHOLOGY

THE NATURE OF THE MIND'S 'FACTORS'

ADDRESS BY
SIR GODFREY THOMSON
PRESIDENT OF THE SECTION

I PROPOSE to devote this address to a consideration of the nature of the so-called 'factors' of the mind, of which we have heard a great deal since Spearman's pioneer work, especially in recent years when questions of selection of children for different kinds of education, or of young people for different vocations, have pressed on our attention. In what I say I may appear to be iconoclastic. But I am not necessarily opposed to the idols I attack. They may turn out to be true gods. I am merely testing them in my mind as Icarus tested his wings. I regularly use, and teach, the very methods of factorial analysis which I am going to criticise, and they have undoubtedly enabled many researches to be made and much insight into the workings of the mind to be gained. But nevertheless they have peculiarities and logical weaknesses which I wish to point out and discuss.

There is a mathematical as well as a psychological aspect of factors and I shall crave the indulgence of a psychological audience if I dwell mainly on the former. The subject is one in which psychologists and mathematicians must co-operate, and I, who make no claim to be either, do however claim to be one who endeavours to understand both, and tries to expound the one to the other. The mathematician is in danger of letting his interest in the numerical and geometrical analogies lead him to build up complicated formulae far beyond the stage which experiment has reached. The psychologist is liable to seize on some of these formulae without thoroughly understanding them, and to use them in ways which shock the mathematician. A certain toleration is necessary if joint progress is to be made. In spite of having spent a great deal of time for many years in studying factorial methods, I find certain difficulties which seem to me not to be frankly faced by psychologists in general but only glossed over and ignored. I find this unsatisfactory and am not without hope that a clear statement of some of these difficulties may either lead to their solution or to a modification of factorial methods.

The decision about the psychological nature of factors is of course one for the psychologist to make, and not for the mathematician. But all the psychologist knows about factors is that they have certain mathematical relationships with real tests, and it is inescapable that he must understand the nature of these mathematical relationships if he is to deduce the nature of the factors from his psychological understanding of the real tests. At present too often all he does is to accept certain coefficients or loadings produced by a dimly understood arithmetical calculation, and base his conclusions on these, without fully realising how mutable and changeable these loadings are, how capable they are of being manipulated to fit different psychological hypotheses or prejudices.

My general attitude to factors of different sorts is that they are useful and vivid additions to our professional vocabulary, but that there is a danger that they may come to be looked upon as much more real and individual mental entities than they actually are. They are, in my opinion, not at all unitary, and only partially innate, being, to a very considerable extent, as I think, built up in the individual mind by many external influences of neighbourhood, school, various experiences and opportunities, availability of books and tools, and such like. They represent, as it were, groupings of these innumerable influences, groupings which may well have considerable overlaps and be intercorrelated. Probably the general level of intelligence, commonly called *g*, is the factor which owes most to heredity, or at least has a 'ceiling' due to heredity, a ceiling which the individual may fail to reach because of environment, but which he cannot surpass. But with other so-called factors I think much more is due to environment. To what, for example, can I attribute such interest in words and such meagre verbal facility as I happen to possess. A village choir in which without knowing it I learned by heart large portions of the Book of Common Prayer may have had something to do with it. So may the fact that one schoolmaster gave me as a Christmas present a copy of Bishop Trench's *English Past and Present*, and another gave me as a prize Brachet's *Etymological French Dictionary* with its interesting long introduction. And so I think it happens with most men. Circumstances give them opportunities or deny them. But their innate talents cause

individuals to seize these opportunities, or to let them slip, so that it is very difficult and perhaps impossible to distinguish the parts which hereditary make up or external opportunity have played in producing present facility.

The whole business of factorial analysis arises out of the calculation, in the first place, of either correlations or covariances which indicate the degree of resemblance between the scores in different tests. There is a very natural tendency to ask what the causes are of these resemblances, and, with I think less justification, to attribute them to a few influences, which have come to be called *factors*, the word factor being used not in the mathematical sense of a multiplier, but in the more general sense, as when we say in ordinary speech ' an important factor in so-and-so's career was the circumstance that he was educated abroad.'

For example, among tests of a cognitive or intellectual nature it is found that practically all correlations are positive, and there is a strong tendency to ' explain ' this by postulating a ' general factor ' ; by which the ordinary man, and I think the psychologist too in many cases, is liable to mean something which *causes* the positive correlations. But it cannot be too emphatically pointed out that the positive correlations may have been *caused* by many different influences and not necessarily by one : and the different influences causing a correlation need not be the same, and probably are not usually quite the same, in the different persons tested. It is really astonishing to find the authors of many factorial analyses saying that they have ' found ' or ' proved ' a general factor, when the fact is obvious that a general factor (in that sense) could be postulated immediately one looked at the correlations. It merely means that the test scores have some tendency to resemble one another. To say that there is a general factor present is mere tautology, only another way of saying that the correlations are positive. Of course there is a general factor in that sense. (Indeed in that sense a general factor is present whether the correlations are positive or negative. But the ordinary man, at least, means by a general factor, one which is available to help in any activity or test, and hinders none, or at least few.)

A spatial model is readily made to illustrate the correlational facts, and such a model clarifies what we are actually doing when we ' analyse ' a set of tests. Each mental activity can be represented by a directed line in manifold space, a central point on the line representing average performance in this activity or test, and performance above or below the average being indicated by points on the line at suitable distances right or left from the average. Two different ' tests ' given to the same population of persons will be represented by two lines, and we can let them cross at the ' average ' point of each, which will now represent a performance average in *both* of them. Furthermore, we can represent the degree of resemblance of the scores in the two tests by the angle at which their two lines are inclined, a small angle meaning that the paired scores in the two tests are rather like one another, a large angle that they are unlike ; and provided the test scores are all standardised, *i.e.* are in units of standard deviation, the lines should be drawn so that the cosine of the angle between them is equal to the correlation coefficient between the tests. Every point on the plane of the two lines can now represent a person whose scores in the two tests can be found by dropping perpendiculars on to the lines, the feet of which perpendiculars indicate the scores.

When, instead of two tests, we have a larger number, say n, these will be represented by n lines in n-fold space, all crossing at a point representing average performance in every test, and at angles with each other whose cosines are equal to the correlation coefficients. Every point in this n-fold space will then represent a person, whose scores in the tests can be found by dropping perpendiculars on to the lines representing them. Since the tests are standardised, the swarm of persons will be spherical in density-contour, and if each test distribution is normal the density will be distributed in normal or Gaussian fashion along any straight line (whether it go through the origin where the test lines cross or not).

In this model, any line through the origin will represent a test, actual or hypothetical. A vector through the origin and through the point representing any person will be the test which that person can do better than any other test. And all the persons on that line, though they will differ in ability, will have similar profiles on the tests of the battery. The tests of the battery are already represented by such lines through the origin, and the sheaf of these actual test vectors is in fact not spread over the whole test space, but tends to be a double cone of that space, a cone whose extreme edges make a right angle with one another. This is the geometrical expression of the fact that few of the correlation coefficients are negative. I cannot of course make an actual model of this arrangement of test vectors, for they exist in manifold space. But I will ask you to think of these lines which I have drawn on the blackboard as being a kind of perspective representation of that n-dimensional model. We are quite accustomed to representing

three dimensional objects on a two dimensional canvas—every picture does this—and here I am, as it were, using a kind of superperspective to illustrate my remarks.

The problem of factorial analysis is to find a set of axes by means of which this model can be described ; and in the earlier years of factorial analysis it was assumed, as obviously desirable, that these axes should be orthogonal, that is at right angles to one another. The mathematician replies at once, of course, that an infinite number of solutions is possible, that any position whatever of a set of orthogonal axes at least as numerous as the tests will satisfy him. The psychologist must then state further conditions to enable a choice to be made among this infinite variety, and he does two things : he explains in the first place that he wants these factor axes to represent psychological entities which he can recognise and name—such as facility with words, or spatial imagery. The axes in any case represent conceivable or imaginary tests, and he wants these imaginary tests to be tests of 'pure' psychological traits. The mathematician responds by teaching the psychologist devices for rotating the orthogonal axes to various positions, among which the psychologist can choose.

In the second place, however, psychologists say they want as few factors as will suffice to explain the correlations or the covariances. This they defend by appeal to the principle of parsimony, the principle that we must not postulate any more entities or any more causes than are necessary to explain the observations. But this so-called parsimony, if parsimony of factors is what is, as I think, usually meant, is really a concealed form of extravagance, for the systems which have as few common factors as possible have in addition, *necessarily* have in addition, as large a 'specific' factor as possible peculiar to each test, so that in total they have more factors than tests, more axes than the dimensions of the test space. This may be desirable or even imperative on other grounds, but parsimony of *factors* can hardly be a proper description of its principles. It may, however, be correctly described as parsimony of non-zero *loadings*, which are fewer in the case of a small number of common factors, even including the specific loadings.

The way in which these comparatively few common factors are arrived at is by inserting what are called 'communalities' in the diagonal cells of the table of correlation coefficients, *i.e.* proper fractions which purport to represent the portion of the test which is common to it and to other tests in the battery. (If variances and covariances are being used instead of correlation coefficients, the full variances are replaced by smaller numbers). The remaining part of each test is explained by a specific factor peculiar to that test alone, at least as long as it remains in that battery.

The communalities which are actually inserted are fractions which will reduce the rank of the matrix to as low a value as possible, or rather, in practice, they only succeed in approximating to a low rank, leaving residuals. The fact that even this can be done means that the correlation coefficients (or the covariances) which are in the off-diagonal cells of the table lend themselves to it, means that this low rank actually rules in fact over the rest of the table. It can be shown that this is exactly what would happen if the causes of the correlations were numerous and small. The rank of this part of the matrix indicates the way in which these numerous causes are grouped together in these activities. If they are not grouped, the overlaps between tests being random, the rank will approximate to *one*, as in a Spearman hierarchy.

The communalities, and the rank of the reduced matrix of correlations, can be determined in the following manner. First of all, an assumption is made of a set of trial communalities, and of a trial rank. With this set of communalities factors are 'taken out' equal in number to the assumed rank, by the best method. The sums of the squares of the loadings of each factor are then found and compared with the assumed communalities. If, as is certain to be the case at first, they do not agree, these sums are used as new estimates and again the same number of factors is taken out. This is repeated again and again until there is agreement between the communalities put in and those which emerge as sums of squares of loadings. Then a significance test is applied to the residues. If they are not found to be sufficiently small to pass the significance test, a larger number of factors must be assumed, and the whole process repeated, the iteration being carried on not with the new extra factors only but with the whole set from the beginning. Ultimately in this way a rank is reached which gives residues not significantly different from zero.

It will be seen that this is a very laborious process involving much iteration. It is in consequence hardly ever performed in its entirety. Workers are content if the communalities they originally guess lead to no very striking absurdities. Under such circumstances clearly no great confidence can be placed in either the number of common factors or the size of the specific factors. And even if all the necessary iteration is scrupulously carried out, clearly the point at which the residues become insignificant will depend upon the number of persons tested.

It is desirable to consider, in terms of our

spatial model, what the effect is of thus inserting fractions less than unity in the diagonal cells of our table of correlations. If unity is left in, and an analysis carried out till no residues at all remain, this means that orthogonal axes are chosen in the test space to describe each person by, instead of quoting his scores in the tests themselves. There will be exactly as many axes as there are tests, and the description of any person by means of them all will be exactly equivalent to the description given by the test scores, neither better nor worse. This means, in our spatial model, that the person's representative point can be equally well reached either by its rectangular co-ordinates on the axes, or by erecting perpendiculars from his scores in the oblique test vectors.

In an actual experiment there is, it is true, always some doubt whether all these dimensions really exist, or whether some of them are not merely due to error in the experimenting. I shall return to this point, but meanwhile, for the sake of clarity, let us imagine that there is no error, that a repetition of the battery of tests will produce exactly the same correlations as before.

The mathematician has various ways of choosing a set of orthogonal axes from his mathematical point of view, and usually he places the first one in the middle of the pencil of test vectors, where it is as near as possible to as many as possible. His second axis, which must be at right angles to this first one, he places so that, if as is usually the case the cone of test vectors is flatter in one direction and more spread out in others, this second axis is in the most spread-out direction. And so with all the succeeding axes, for we must remember that we are operating in a space with as many dimensions as we have tests. He takes axes in this sequence because, for the purpose of describing the positions and directions of the test vectors, this is their order of importance. In the language of the factorial analyst, this is the order in which they ' take out ' the maximum amount of variance of the tests at each step. They are all of them, however, equally necessary for the purpose of describing a man, for remember that in our model the points representing persons are distributed spherically in our space. I think that many psychologists perhaps fail to appreciate this.

Some psychologists tell us that they carry out factorial analyses only for the purpose of classification, though it is not quite clear to me whether they mean classifying the tests, or classifying activities of the mind—perhaps there is no difference. Such psychologists are usually, it would seem, content to accept axes like the above without any rotation, and I agree that this seems justifiable as a method of classifying the tests of this particular battery into different sorts. If that is the object, I would imagine that the analysis should be carried out, as I was assuming in the previous paragraph, with unity in each diagonal cell. For then the classifying axes would be in the same space as the test vectors they are classifying.

Usually, however, even classificationists insert the lowest possible communalities in the diagonal in an endeavour to reduce the rank of the matrix. Let us consider what the effect is of putting any fractions less than unity into the diagonal—not necessarily the lowest possible fractions.

The moment this is done, we have allotted a specific factor to each test and determined its size. We have already, that is, performed a very important part of the whole analysis. No doubt most tests have something specific or peculiar to themselves. But it appears to be a very important decision for the psychologist to make, to say (or rather I fear not to know he is saying) that he will allot to each test as big a specific factor as the mathematician will allow, which is what he actually is doing when he puts in the minimum possible communalities.

Further, as soon as any fractions, minimum or not, are placed in the diagonal cells, we have thereby decided to use, in describing our tests, more orthogonal axes than there are tests : for each test has then a specific factor, and there are in addition the common factors. This means in terms of our spatial model that none of the axes, neither the common factors nor the specific factors, are in the test space at all, (except at the origin where they all cross). It is only about the test space, of dimensions equal to the number of tests, that we have any information from our battery. These axes are away in outer darkness and we cannot know them, but only their projections or shadows on the test space. Psychologists invariably confine their attention, after making an analysis using communalities, to the ' common factor space,' of a comparatively small number of dimensions, without, I think, being usually aware that this space is not in the test space at all (Thurstone's ' secondary factors,' in their turn, are not even in the common factor space, for he uses what I might call secondary communalities). The effect of all this is that the factors arrived at by an analysis which has begun by placing fractions in the diagonal cells can never be measured in any man, but only vaguely estimated, and with maximum vagueness if minimum communalities are used.

In itself the fact that factors can only be estimated and not accurately measured is of course not fatal. Throughout statistical work runs the idea of estimation in a realm

outside that which is experimentally known, in a realm of more dimensions than that in which our measurements have been made. It is to allow for that that the device of 'degrees of freedom' is used in the analysis of variance. But in factorial analysis the vagueness due to estimation is deliberately maximised.

A certain amount of vagueness is no doubt inevitable. For example, I have earlier mentioned the fact that the tests are 'unreliable,' that is that a test's correlation with itself, when it is repeated, is not perfect. There may be justification for placing this self-correlation in the diagonal cell before analysing, though I am not fully convinced even of that: for to do so implies that the errors or variations in performance are not in the test space at all, except in the one test to which each is attributed as a specific. Especially if the tests are given on the same day, or even in the same week, influences which cause a man to be in good form or in bad will usually act as a common factor influencing several tests. However, as I said above, the unreliability of a test is to a very considerable extent remediable, and should be reduced to a point where its influence is not important. Even if there were no unreliability, it may well be urged that every test has a specific factor peculiar to itself, uncorrelated with any other test. In that case there is justification for reducing the unity in the diagonal cell by some amount. But is there justification for reducing it as much as is mathematically possible, is there justification for leaving the decision about the size of the specific factor entirely to a mathematical principle, leaving the psychologist no say?

I have spoken so far of the desire to explain the correlation coefficients by as few common factors as possible, and I turn now to the psychologist's other condition, that the factors should be not merely mathematical coefficients, but recognisable as psychological entities. He has I think already somewhat jeopardised the attainment of this by his use of *minimal* communalities, which results in his common factors being in a space very remote from that described and defined by the tests; but since he seems to insist on that let us fix our attention on his common factor space, a space of fewer dimensions than the test space, containing the pencil of communal components of the tests.

The test vectors formed a double cone in the wider space, at angles depending on the correlations. The communal vectors, crowded more closely together in the common factor space, also form a double cone; and because they are more closely crowded together, the angle between the extreme edges of the double cone, which in the test space was about a right angle (since few correlation coefficients are negative) will in the common factor space be less than a right angle. The mathematician's common factor axes are orthogonal to one another in that space, the first down the middle of the cone, the others at right angles to it, and to each other. As long as mere classification of the tests is what the psychologist is after, it would seem to be best not to rotate the axes first arrived at (usually centroid axes) or at least to be unnecessary to rotate them, though I do not think they should be called psychological factors.

But if psychologically recognisable entities are what is wanted, and if the psychologist wants to arrive at tests which are as near as possible pure tests of a single factor, then it seems certain that the axes should be rotated away from their centroid positions. For consider again where the centroid axes are situated in relation to the double cone of communality vectors. The first centroid factor is right in the middle of the double cone, and in its case it seems not inconceivable that an actual test could be discovered, by search, to coincide with it, or to do so very nearly. But all the other centroid axes are sticking out from the apex of the double cone into an equatorial region where there are no real test vectors at all, and it is perfectly hopeless ever to expect to find a real test to coincide with, or even to approximate to, any one of these later centroid factors. What can be done, however, and what many American psychologists confidently do, is to rotate the whole set of orthogonal common factor axes into a position where they enclose the double cone. They will then be just beyond the periphery of that conical region wherein real tests are found, and have a reasonable chance of being approached by some of the real tests. Such a rotation can be readily approximated to by postmultiplying the table of centroid loadings by an orthogonal matrix whose first row is composed of elements each equal to the reciprocal of the square root of the number of common factors. There is an infinite number of such orthogonal matrices of given size, so that the factor axes, which are then all equally inclined to the centre line of the cone, are not yet in unique positions, nor need they be kept at exactly equal angles to that centre line. But being only a little beyond the periphery of the cone, they have in any case a chance of being at least approximated to by a real test, or rather by the communal part thereof. No one of them, however, will now be singled out as a *general* factor. Each of these factors may be absent from some of the tests, or at least present only with very small loading.

When British psychologists rotate their centroid axes, however, they do so in a half-

hearted way, not moving the first centroid factor too far from its original position. Though no longer at the centroid, it is left still inside the boundary of the double cone or at it, as a general factor, and the other orthogonal axes are in consequence likely to be a little further removed from the cone of reality, a little less likely to find representation by 'pure' tests than the above American axes, as I may call them.

In either case, however, since the angle of the cone is an acute one, but the axes orthogonal, the axes, or at any rate some of them, are bound to be outside the cone. If therefore the desire to have 'pure' tests of the factors is strong, it is clearly necessary to close the factors somewhat together, in order to bring them up to the periphery of the 'real' communal vectors. That is, factors likely to be susceptible of representation by 'pure' tests are likely to be oblique, to be correlated with one another. It will be noted however that this argument in favour of obliquity of factors hangs together with the use of minimum rank and a common factor space of comparatively few dimensions. The acute angle of the double cone is due to its compression into this narrower space. In the full battery space orthogonal factors which are real are more attainable.

American psychologists have shown no hesitation in accepting and using oblique or correlated factors, despite the statistical difficulties to which they give rise—which are, however, offset by the fact that statistical calculations and battery weightings, etc., are less necessary if nearly pure tests of factors can be found. British psychologists cling to orthogonal factors. Yet they too admit oblique factors by a backdoor. For it is quite common to hear *gv*, and *gk*, spoken of as though they were entities : that is to say, although three orthogonal factors *g*, *v*, and *k*, are postulated—the general, verbal, and space factors—actually two oblique factors are used. And it is recognised that tests can be made for *gv*, or *gk*, although tests for 'pure *v*' or 'pure *k*' are probably impossible. 'Pure *g*' might be possible.

But these two requirements of the psychologist, to have only as few common factors as possible, and yet such that 'pure' tests of them can be found, are interrelated. For even if the common factor axes should be rotated within the common factor space until they are in positions where actual communal vectors may be expected to occur, it must be remembered that the real test will have its specific factor, and that the process of reducing the matrix of correlation coefficients to as low a rank as possible is equivalent to assessing the specific factor of each test at as big a value as possible. A so-called 'pure' test, therefore, though it may be free from other factors of the system if these are orthogonal, will nevertheless have a big specific factor. It is as though we called a beakerful of weak sulphuric acid 'pure sulphuric acid' because it contained no other acid, ignoring the fact that it was much diluted by water. Most 'pure' tests of *g* in fact have a smaller loading with *g* than good verbal tests.

I would like for a while to direct attention to these specific factors which apparently dilute all real tests. What is *their* nature? Possibilities which will occur to us are first, that they are error factors—but errors or variations in the subject's performance are, as I have already said, not likely to be entirely unique to one test. Secondly, they have been attributed to sampling errors in the coefficients of correlation—but these sampling errors are themselves correlated, and so give rise to false common factors, not to specific factors. Thirdly, they may be real mental factors, unique to that test, needed only by it. But what remarkable consequences follow if we accept that. I devise a brand new test and, lo, in the mind of man there exists a specific ability to do that test, and moreover an ability which is useless in every other activity. Further, every individual I meet possesses this specific ability in large or small amount. The idea in this form is really fantastic.

It would seem then that the specifics cannot be really unique, but only unique in this battery. This leads to the presumption that the tests of a battery only possess specific factors because there does not happen to be in the battery any other test to share the specific, or at least part of it, and prove it to be really one or more common factors. On this view, specifics will disappear when a test has been tried in a large number of batteries, or in a sufficiently large battery. Not only does this seem unlikely when one considers that in every battery the minimum communalities and maximum specifics are insisted on, but it also has peculiar consequences in regard to the number of primary factors. Consider a battery consisting of, say, two dozen tests, analysed into, say, seven common factors plus of course two dozen specifics. The latter, it must be remembered, are all orthogonal, all uncorrelated with one another. On the hypothesis that they are really factors which just do not happen to have found a partner, like wallflowers at a ball, there must exist at least two dozen other primary factors waiting to be discovered in a larger battery. And so with every battery of tests. The number of primary factors must be larger than all the tests hitherto invented, which does not seem to be parsimonious. I cannot help fearing that there is something wrong with the idea of reducing the matrix of correlation coefficients or covariances to its lowest possible

rank, and then calling the descriptive variates to which this leads ' factors of the mind ' : something wrong with the whole idea of attributing as much as possible of the variance of a test to a unique factor, something wrong with the ' parsimony ' argument upon which all this is based. It leads to too many difficulties to which it is possible, but not I think advisable, to shut one's eyes. Moreover, the reciprocity principle, which identifies factors and loadings obtained from correlating tests with loadings and factors obtained from correlating persons, only works when there are no specifics involved. I would like to see a number of existing squares of correlation coefficients re-analysed with full variance in each diagonal cell and the results considered. There would be no guessing of the communalities, and no repetitions or iterations of the calculation to determine them. Tests of significance of residues would be more easily made, and although rather more factors would be necessary before the residues became insignificant, they would have the advantage of more accurate estimation in any man. True, such factors would be confined to the particular test space of that battery, and admittedly a factor of the mind is not likely to be an exact composite of the tests of any one battery. But the point is an academic one, for to appear to determine the position of a factor of the mind in a space of more dimensions than that which we have investigated is self-deception. Besides, the common factor space in which communality factors exist, is just as much a creation of the particular battery as are axes determined within the battery space.

I must not be misunderstood as saying that no specific factors exist at all. What I am sceptical about is the procedure of making the specific factors in every battery as large as possible, by the automatic application of a mathematical device. That every test may well have some unique quality for any individual person seems conceivable, though I do not think this special feature of the test will be felt as a peculiarity by every person who tries the test. I think any such unique quality would be a blemish in the test, just as unreliability is a blemish, and that the psychologist should endeavour to make tests which are neither unreliable nor burdened with unique peculiarities. Probably he cannot avoid a certain amount of uniqueness, just as he cannot avoid a certain amount of unreliability. But I do not see the need for ascribing *maximum* uniqueness in order to reduce the number of common factors. Let me return again to the parsimony argument.

That argument, as usually stated, asserts that by the use of communalities the correlations or the covariances are explained by the smallest possible number of factors. But that is not so. For the common factors alone do not explain them. Of themselves the common factors would give correlations higher than the observed. The specific factors are needed to dilute the correlations down to the observed values. And although the inner product of the common factor loadings will give the observed covariances, these are meaningless except in relation to the variances. If the observed variances had been equal to the communal variances inserted in the diagonal cells, the covariances would also have been different. All the factors, greater in number than the tests, are needed to give the observations.

But these observations could have been explained by fewer factors. Certainly no more are required than the number of tests. And if (as is done in the other process) the extraction of factors is stopped when the residuals are small, still fewer factors would suffice.

But, it may be replied, if even small truly unique parts of the tests are admittedly present, there will always be the need for the large number of specifics. Possibly so—but specifics of no great importance, if the tests are good ones, specifics with an influence as unimportant as the causes are of the residuals which we in any case ignore after statistical testing.

It is true that by the use of communalities the total number of *loadings* to be estimated is reduced to a minimum. That way of putting the parsimony argument is perhaps defensible. What I doubt is whether too high a price is not paid, since this same procedure maximises the specifics, and decides their importance without any psychological consideration whatever being given to the question.

The practical conclusions I would draw from these considerations about the nature of specific factors are that a battery used for factorial analysis should be composed of tests of high communality in that battery: or that, if tests are admitted which by the mathematical principle of rank reduction are allotted low communalities, the psychologist should agree that these tests do draw, each of them, upon factors of the mind not represented elsewhere in the battery. I am, of course, assuming that the tests are reliable to a high degree.

My own opinion about the causes of correlations between tests is one which I have already hinted at in this address, and have often developed in previous publications. It is that these causes are very numerous and not usually of individually outstanding importance, though that may happen in certain cases. Some of the causes which enable a man to achieve a high score in a given test are probably genetic, some due to early education or to recent experience or stimulation, some

due to accidental or more fundamental points of interest, due to what you will. Some of these will also help him to do well in a second test of somewhat different nature, some of them will not. Other different causes and influences from his past or from his genetic constitution will come in to help in this second test. The correlation, or the covariance in relation to the variances, expresses the amount of overlap between the two groups of large numbers of small influences or causes.

It has been shown both empirically and mathematically that such a theory will lead to a tendency to low rank in the matrix of correlation coefficients between many tests taken in pairs. Indeed, were the tests very numerous and each of them composed of, or soluble by, a random selection of the small influences, the matrix of correlations (in its off-diagonal parts of course) would tend to rank one. It would not be absolutely of rank one, but the residues after removal of one Spearman common factor would be relatively less and less important as the number of tests was increased, provided each of them drew on a random sample of the above postulated small influences assisting their solution. Only few of the numerous causes would actually be common to all the tests, only few of them would be confined to one test.

This is in effect a restatement of Spearman's Theory of Two Factors, with an important difference. The correlations produced by this set of underlying causes would, it is true, act *as if* they were due to one common factor and a large specific factor in each test. But my theory does not suggest that such a common cause, or such specific influences, actually exist as realities in any sense either physiological or mental. The real causes, the theory suggests, are much more numerous, and moreover may differ from individual to individual. The clinical worker with individuals must be called in to co-operate with the statistical psychologist and put a brake on overmuch generalisation.

The rank would only tend to rank one if the tests drew upon random samples of the numerous underlying influences. Usually that will not be the case. Tests will fall into groups—verbal, spatial, perceptual, what not. The tendency to rank one will be checked by this, and only a tendency to a low rank will remain. The correlations will be related *as if* they were due to a small number of common factors (no longer one only) and a specific in each test. But again from my point of view these ' factors ' are not unitary entities. They arise naturally and mathematically from a theory of the overlap of many and much smaller realities.

This theory is admittedly not parsimonious of causes. It thinks they are very numerous indeed, and as it happens the postulation of such a large number of small causes is actually equivalent, mathematically, to the assumption of a small number of common causes plus specifics. Just as the interplay of many small causes can produce a Gaussian distribution, so the interlocking and overlapping of many small causes can produce among correlation coefficients a relationship expressible by a small number of common factors plus large specifics.

Perhaps I may be permitted to employ a humbler analogy. Three charitable collections have been made in Newcastle for three different objects and have produced, let us say, £200, £300 and £400. A parsimonious theory of the number of contributors would be to assume that one general contributor had given £100 to each charity, and that three other specific contributors had each of them given a sum to one charity only, to make up the balance. The theory is fully compatible with the facts known. But actually it is far more likely that dozens, scores, hundreds of people had given their sixpences and pennies, some of them no doubt to more than one, some to all, some to a single charity. We can explain correlation coefficients by a few common factors plus specifics, but it does not necessarily follow that these factors are any more real as causes than are the few hypothetical rich contributors. It seems to me far more likely that the real causes are much more numerous, and that they differ from man to man. It is the grouping of these causes which results in our ' factors,' and how they are grouped will depend almost entirely upon the civilisation in which the tester and the testees live. Some of the ' factors ' may be more fundamental. Some, such as the perceptual factor, may, I think, perhaps be reasonably independent of a particular culture. But a verbal factor and a space factor are both, I think, creations of our culture. In a different cultural environment the tester's tests would change and the testees would think differently. I can imagine in a primitive community an archery factor, or a cattle-judging factor, being discovered and, were I a humorist, I could write an account of experiments in such a community carried out to decide whether the archery factor, which undoubtedly existed at age 16, could be detected and measured as early as 11+.

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SECTION K.—BOTANY

THE UTILISATION OF THE MACROSCOPIC MARINE ALGAE THROUGH THE AGES

ADDRESS BY

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PRESIDENT OF THE SECTION

THE macroscopic marine algae fringe the rocky shores of the lithospere and extend into the adjacent comparatively shallow water. There is abundant evidence that they were used by coast dwellers from very early times for a variety of purposes but the quantitative curve of utilisation has altered periodically and must be viewed against a background of economic history, if its changing form is to be fully appreciated. In 1949 we are witnessing what I hope will be a permanent rise in the curve, when modern transport and machinery are making seaweed products available even to inland populations. Despite the variety of these products, the raw materials on which they depend are the marine algae themselves. As botanists, therefore, we ought to pause and reflect on some historical aspects of the subject and try to trace the story of algal utilisation through the ages. The White Queen reminded Alice that it was a poor mind that only worked backwards, and having looked back perhaps we can also look forward and see the lines along which future work can best be directed.

To try to convince my fellow-botanists of the usefulness of the marine algae would doubtless be to preach to the converted, but there is abundant evidence that a section of the community, through the ages, has despised these denizens of the shore and shallow seas. As early as 23 B.C., Horace in one of his Odes described an approaching storm which 'down sweeping from the East shall strew the grove with leaves, with *useless wrack* the cove.' Virgil and others, writing at about the same time, used *vilior alga*, worse than seaweed, as an almost proverbial term of supreme contempt. Seaweeds continued to be despised in the writings of literary men and as late as 1733 Pope wrote in the *Essay on Man* :—

'Mark by what wretched steps their glory grows,
From dirt and seaweeds as proud Venice rose.'

Nevertheless, the use of the algae had begun much earlier than the lifetime of even Horace and Virgil, and it was also contemporaneous with them.

It is to Chinese history, which goes back at least to 1520 B.C., that we must look for the earliest oriental references to the algae. Twelve species were described in *Erh Ya*, the oldest known Chinese Encyclopedia written earlier than 300 B.C. in the Warring States period of the Chou dynasty. In the earliest Chinese herbal written about 300 years later but now lost as a separate book two species of Sargassum were mentioned. Chinese physicians used seaweed for treating goitre from very early times and the agar weeds (three species of Gelidium and one of Gracilaria) were described as early as A.D. 300. A little later five species of Porphyra were mentioned, and all these plants have continued to find a place in Chinese literature to the present day. The agar weeds and Porphyra species were evidently used very early as food, and had popular Chinese names. The occurrence of popular names for algae continues, in all regions of the world, to mark the genera and species which are commonly harvested.

In the Roman Empire seaweeds served a number of purposes. Their medicinal value was recorded by Nicander and by Pliny the Elder, in the first century A.D. or earlier. Ovid, Pliny, Nicander and Quintilian all mentioned the value of seaweeds for dyeing, though Quintilian recorded that cloth dyed with seaweed was entirely outclassed by a cloak of Tyrian purple. Pliny also recorded the use of seaweeds as the source of a fast dye and a mordant. This interested me very much because an old man at the Talybont woollen mills near Aberystwyth told me that his grandfather collected seaweeds from the shore for dyeing woollens.

There is abundant evidence that the Romans used the larger algae as manure for cabbages, pomegranates and citron, and for cattle-fodder in times of scarcity.

At this point I must acknowledge my indebtedness to those who have so kindly helped me to steer my barque in the algologically uncharted seas of Chinese and Latin literature.[1]

The use of seaweeds as food in the east and as fodder and manure in the west foreshadows later developments.

In the coastal regions of parts of the eastern Mediterranean, Spain, France, Great Britain and Scandinavia the use of seaweed in agriculture appears to have developed very early. Domestic animals have fed from time immemorial on the drift-weed or nibbled accessible rock-weed, different algae being preferred by different animals. Records and observations show that *Rhodymenia palmata*, *Chondrus crispus*, *Alaria esculenta*, *Pelvetia canaliculata* and the young fronds of *Laminaria saccharina* are freely eaten by cattle, horses, sheep and goats. In the Hebrides, cattle, feeding on the shore, before and immediately after the turn of the tide, are still a familiar sight. On North Ronaldsay, the most northerly outlier of the Orkney Islands, where grazing is particularly scarce, the drift weed forms an important part of the total food of the sheep. In many other areas, halophytic saltings, in which small Fuci form a part of the sward, are found to provide excellent grazing for sheep.

In addition to such free-range grazing the *gathering* of seaweed for fodder was widespread in the coastal areas of Western Europe before the production of root-crops and the availability of cattle-cake increased the succulence and nourishment of winter food. The Fuci were boiled and the ' brae ' poured on hay, chaff or oat-husks ; alternatively fresh Fuci were stored between layers of hay and used as silage. Pigs were fed on Sargassum in China, though the general utilisation of the seaweeds as fodder was a more prominent feature of western civilisation. In recent years, the value of seaweed as stock food has been recognised in the U.S.A. and to some extent in New Zealand.

In order to extend the area over which seaweed could be used profitably, modern methods have been devised in Eire, Scotland, Norway and the U.S.A., to produce ground seaweed which can be transported easily and converted into a balanced ration by the addition of other foods. Such meals, made chiefly from the Fuci and Laminarias, are said to be especially favoured not only for cattle but for foxes bred for their pelts. On the whole the algae are better for ruminants than swine and they increase the iodine content of milk and eggs when fed to dairy cattle and poultry respectively. Research has been conducted, in all the countries concerned, on various aspects of the digestibility and palatability of the larger brown seaweeds. The largest constituent is probably alginic acid, in addition to which there occur laminarin and proteins all highest in spring, as well as fucoidin and mannitol, the last being highest in August. The work of Lunde and others indicates that domestic animals can assimilate the available carbohydrates better than the proteins and that the feeding value of Laminaria is on the whole higher than that of hay in spring and lies between the values of hay and oats in autumn. It must be recognised however that laminarin is not acted upon by common enzymes, and the only one known to be capable of hydrolysing it occurs in the intestinal juices of the snail and the limpet. The salts which the plants contain can of course be assimilated, though their presence imposes a limit on the total quantity of seaweed that can be used safely. According to Senior, Kelly and Collins, it should constitute no more than one-third of the total feed, and Fucus is of less value than Laminaria.

Recent research by a number of workers has shown the brown seaweeds, especially the male gametes, to be rich in α and β carotin, a proportion of which would readily be converted by the animal into Vitamin A. Vitamin B_1 and B_2 have also been isolated, together with Vitamin C, the latter occurring in littoral seaweeds. Vitamin D has been shown to occur by Tilden, and Fucosterol, a compound close to Vitamin D, has been isolated from many seaweeds by Heilbron and his co-workers.

In addition there may well be a number of ' trace ' elements present which would have considerable fodder value. The gelatinous nature of the plants would also be of mechanical value in promoting peristalsis.

In spite of these favourable discoveries, seaweed has found little use as fodder in this country. It is usually regarded either as a filler or as a means of providing salts and roughage.

Seaweeds appear to be palatable to domestic animals when once they have become accustomed to them, though horses, sheep and cattle when offered algal fodder for the first time, need a few days of habituation before they can digest it, even after they have been induced to eat it.

When the art of crop husbandry spread from the plains of Asia all over the world, the inhabitants of Britain, as well as their continental neighbours, acquired some knowledge of tilling and manuring. They had learned to add seaweed to the soil, even before the Roman Conquest. Agriculture declined under the Saxons but improved about the middle of the tenth century. In a

[1] Dr. Joseph Needham (Cambridge), Professor E. D. T. Jenkins and Mr. L. N. Wild (Aberystwyth).

manuscript on the history of Pembrokeshire dated 1560 an account is given of collecting driftweed for manure and it is recorded as 'serving for one year only' and not 'a durable amendment.' This is the early realisation of a fundamental fact that recurs constantly in later literature. Legend and narrative concerning the manuring of the land with sea-ware are scattered throughout Welsh and Scottish early agricultural records but by the middle of the eighteenth century both agricultural and manurial practice had changed very considerably in Britain. The Norfolk four-course rotation had been established and the use of turnips had spread from East Anglia as far as East Lothian and Anglesey. This led to the folding of sheep and the feeding of both turnips and hay to stalled cattle. As a consequence farmyard manure increased and the need for seaweed was less acute. The practice of harvesting both drift and rockweed continued however, to some extent, especially in the coastal districts of Ireland, Wales, the West of Scotland and the Western Isles, as well as in France. It was especially valuable for barley but its effect varied locally when used for potatoes and oats. On the whole it seems to have produced the best results on light land. In Ireland, the Fuci are still grown on prepared boulders for use as manure, *F. vesiculosus* and *Ascophyllum nodosum* being the favourite crops. Recently a company in Johannesburg has been given the right to cultivate seaweed from parts of the South African coast for use as a fertiliser.

In Japan, a relatively small quantity of the coarse weeds that are no use for human food or for other profitable commercial purposes, is used for manure on the rice fields, and in China species of Sargassum are used similarly for potatoes, ground nuts, coconut, coffee and other purposes. Direct application of seaweeds is also used for coconuts in Ceylon and for agricultural crops in Alaska.

In addition to this direct application, seaweed was used for making compost, together with shelly sand and dung. The addition of compost was found to be of more lasting benefit than the use of seaweed alone. This is of particular local interest to me because there is evidence that in Anglesey, in the seventeenth century, the shelly sand was derived from the kitchen middens of an earlier civilisation. These had accumulated in large quantity when the population of that island used molluscs in their staple diet and the remains of some of these middens can still be found. In some places in France, Ireland and Scotland the shelly sand was obtained from the shore where the so-called coral strands, formed largely of shell and the calcareous alga Lithothamnion, made an excellent composting material. The Lithothamnion contains as much as 40 per cent. lime in addition to potash and nitrogen, so that its use added greatly to the value of the compost.

In a few areas, such as the Scilly Islands, extensive harvesting of seaweed for manure continues, but in most instances the collecting, carting and composting of wet weed which was a routine labour in the days of subsistence farming is no longer deemed practicable in this era of mechanised agriculture.

Advances have however been made to some extent in that in some areas the weed is burned and the ash applied to the soil. There is some loss of nitrogen, though not of the valuable potash salts, in this method, but there is a gain in facility of transport. In Massachusetts seaweed has been ground for manuring tobacco, and in Eire and Norway for use as a general fertiliser.

In recent years, two wars have encouraged renewed scientific investigations on the value of seaweed in agriculture and horticulture. To these researches, both Eire and Great Britain have made substantial contributions. As a result, the facts that Laminaria is better than Fucus, that frequent applications are necessary, and that the potash salts leach out very easily, have all been put on a statistical basis. It has also been shown that the adverse effect of sodium chloride, for some crops, can be minimised by applying the weed in winter. Work on the value of seaweed in horticulture is still in progress in Britain by Chesters.

In addition to the value of the mineral content of the brown seaweeds, including trace elements, they also increase the power of water conservation in light soil and they have the advantage of being free from the weed seeds and fungus spores which may cause various crop diseases if farmyard manure is applied.

I have dealt so far with the use of the larger brown seaweeds in agriculture, either wet, dried or burned. The next phase of the seaweed industry was the extraction from the algae of soda, potash and iodine. From the seventeenth century, crude soda for the glass and soap industries had been exported from Spain and the Levant in the form of barilla, which was the ash obtained by burning halophytes. When it was discovered that a similar substance could be obtained from the Fuci and Ascophyllum, and that the presence of additional sodium chloride was not a disadvantage, an industry for its preparaton grew up in Northern France where these seaweeds had formerly been used as manure. The practice spread northwards to the Channel Islands, Ireland, the West of Scotland, Norway and Sweden. The burnt

solidified ash prepared in primitive kilns on the shore was known as kelp, and by the beginning of the eighteenth century its production was an important feature of the West of Scotland and the Western Isles where many of the burners were crofters. They made kelp from early June to the end of August and tended their crofts between whiles. The landowners received considerable royalties and the prosperity of the area soared during the American War of Independence (1755–1783), when barilla was not available and the price of kelp rose to £20 a ton. A second impetus to production came early in the nineteenth century because of the shortage of barilla during the Peninsula War and of the timber famine in Europe, where metallurgy and mining had caused the cutting down of the forests which had been previously an important source of alkali.

Later, a series of economic changes led to a decline in the industry. The import duty on barilla was lowered, the salt tax was first reduced for fish curers, and then repealed entirely, and also the Stassfurt mineral deposits were discovered. In addition the Le Blanc process for the production of soda from common salt was introduced into Britain by Muspratt. The discovery of the Stassfurt deposits considerably reduced the price of both potash and bromine. At best, kelp was an expensive source of soda and it could not compete in price with more fruitful sources of alkali.

Another revival of the kelp industry came later, however, with the discovery of iodine. During the Napoleonic wars France was unable to import saltpetre for gunpowder owing to the British blockade, so it was made artificially by using wood ashes as a source of potassium. Courtois, a native of Dijon, found a cheaper source of potassium carbonate in kelp. Subsequently, when cleaning his kelp-burning apparatus with strong sulphuric acid, he observed a beautiful violet vapour coming out and condensing on the cooler parts of the vessel. The new substance was named iodine and it was found that Laminarias from the sub-littoral zone contained as much as 0·5 per cent. of it in their ash. For many years the Phaeophyceae were the sole source of this new substance, six centres for its manufacture being set up in France, while by 1846 Glasgow had become the British centre of the industry with twenty factories.

The selection of seaweeds for harvesting was greatly changed by the discovery of iodine. Though the rock weeds were richest in soda, a higher iodine yield was obtained from the Laminarias, which form the forests of the sub-littoral zone to a depth of about twenty fathoms. Laminarias from the drift-weed could also be used, a matter of great importance to the wind-swept west of Scotland and Ireland, where enormous quantities of stipes and fronds are cast up in winter and in early summer respectively.

In 1863 E. C. Stanford, a man of great distinction in practical algological research, introduced two new manufacturing processes. One of these was the ' Char ' process, whereby the dried seaweed, after distillation in iron retorts, produced illuminating gas, heavy oils and ammonia in addition to potash, bromine, iodine and charcoal. This did away with some of the wastefulness of older methods, and had the further advantage that the weed could be collected all the year round and dried in sheds. Its disadvantage was that the necessary equipment was expensive and the apparatus needed skilled attention. Stanford also introduced a method by which the winter-cast stipes of Laminaria were carbonised in closed retorts, thus simultaneously making charcoal and collecting the products of destructive distillation. Many rival processes were instituted and these have continued to appear sporadically despite important changes in the commercial position.

Though improved methods benefited the kelp industry for a time, the final blow was dealt to its fluctuating fortunes by the discovery that iodine occurred as iodate in the crude nitrate of soda deposits of North Chile. North-west Europe, which had been the centre of world iodine production, inevitably felt the worst repercussions of the new discovery. In the western isles of Scotland a prosperity which has never since been equalled gave place to dire poverty and emigration. Despite low prices, however, the production of kelp has never ceased completely either in France, Scotland or Ireland, and little primitive kilns may still be seen, scattered around the coast, though nettles often partially envelop them. The total output, however, is now negligible though it revived temporarily between the world wars owing to a shortage of potash.

During the latter part of the nineteenth century, when the European industry had fallen almost to zero, Japan and the U.S.A. began to harvest Phaeophyceae, stimulated by the temporary rise in the prices of potassium salts and iodine. With this geographical extension of the industry the term ' kelp ' came to be used for the seaweeds themselves as well as for the ash. Japan had imported potash salts and iodine from Britain up to 1885, but with her great seaweed resources she soon became an exporter, and from 1909–1916 she exported extensively to many other countries. Species of Laminaria, Ecklonia and Sargassum were used, and it was ascertained that plants from the open sea had a higher iodine content than the same species

harvested from an inland sea, and also that the yield was highest in autumn.

By 1912 the U.S.A. was producing kelp ash from Laminaria species on the Atlantic Coast and from the great beds of Macrocystis and Nereocystis in the west, which harbour an immense animal population among their extensive thalli. Mechanical harvesters had been devised and suitable areas surveyed and charted.

Meanwhile Sweden continued to produce a certain amount of kelp, Canada was marketing it up to 1942, and Russia had discovered a new process, which made use of a member of the Rhodophyceae, *Phyllophora nervosa*, from the Black Sea. This plant after maceration in water and subjection to electrolysis, with a rising potential, yields iodine, bromine and chlorine from the anode as well as mannite after concentration ; cellulose and alginates are obtained at the cathode. It is not known whether the process was used commercially.

In 1884 E. C. Stanford gave a paper to the Society of Chemical Industry in London, in which he announced the isolation from the brown seaweeds of a new substance to which he gave the name of algin. This discovery was of major importance in the history of practical algological research and seaweed utilisation. It marks a change of emphasis in commercial production from inorganic to organic substances.

Stanford observed that Laminarias in the drift swelled when wetted by rain, and that the swollen patches contained a fluid which adhered to the fingers and could be drawn out into threads. He found this same substance could be extracted from the residual material after the plants had been first macerated in dilute hydrochloric acid and then soaked in sodium carbonate. In twenty-four hours the whole plant broke up into a heavy gelatinous mass which contained algin in the form of sodium alginate. Stanford recognised the commercial possibilities of his discovery and made extensive investigations into the properties of the new substance and its possible uses. He renamed the compound ' alginic acid ' when he found that it formed salts with metals and liberated CO_2 from sodium carbonate. Alginic acid is an important constituent of the cell-wall of the Phaeophyceae and is mainly responsible for the tensile strength and elasticity of the plants. There has been much subsequent research on the nature of the compound and its mode of occurrence, as well as its seasonal variation in different seaweeds.

Among the algae of Western Europe and Britain, the Laminarias, Alaria and Himanthalia are especially rich in alginic acid, but their location and large-scale harvesting present major problems because the main crop is produced on a rocky substratum and totally submerged in the sublittoral zone. Extensive harvesting of winter-cast stipes is relatively easy, as is also the collection of the cast fronds when they are conveniently deposited on the shore. The presence of this material, known as ' May cast,' is subject, however, to vagaries of wind and tide that cannot be predicted beforehand.

In the U.S.A. alginates are produced on the Pacific coast from *Macrocystis pyrifera*, which floats in immense masses on the surface of the water, though it is attached to the substratum. These vast tracts of raw material are therefore not only easily located but harvesting can be carried out mechanically, two facts which greatly benefit the competitive economics of the industry. Japan has not so far been a serious competitor in the world's alginate market, and at present she appears to be concerned mainly with other uses of seaweeds. Australia has great resources of the large Phaeophyceae, and research on the possibility of alginate production is already in progress there. The suitability of an area for large-scale alginate production depends on local conditions. These latter include the ready location of the plants, the possibility of mechanical harvesting and a supply of cheap labour. Harvesting is further facilitated if the coastal population is accustomed to dealing with algal crops on a large scale.

The technical importance of alginic acid, from whatever seaweed it has been obtained, is based principally on the properties of its salts. The salts of the alkalis and of magnesium dissolve readily in water and give solutions of very high viscosity which neither coagulate on heating nor gel on cooling ; in addition they have a wide range of controllable viscosity. The heavy metal salts of alginic acid are insoluble in water. They form plastic material which can be moulded when wet but which becomes very hard on drying and very difficult to bring again into solution.

The commercial value of the salts is high and their uses are varied. Like other seaweed colloids they can be used in the textile and leather trades, in pharmacy, medicine, surgery and the food industry. They are also of great value for making plastics, foil and alginate yarns. These latter have a variety of uses, some of which are concerned with the weaving value of the yarn itself and some of which depend on the ready solubility of certain alginates, that can be removed easily from the completed fabric, when their purpose is fulfilled. These soluble alginates may carry the thread twisted around them, and when they are dissolved after weaving, a

crinkled or caracol-like fabric results. Lace is also made on an alginate base which is dissolved subsequently. The use of some of the soluble alginates as mordants for dyeing recalls their early use for that purpose by the Romans as well as in Crete and Turkey.

In view of the commercial importance of the alginates, research on various aspects of the acquisition of the seaweeds which yield them and their subsequent extraction is in progress in Britain, the U.S.A. and Australia. In Britain, Knight and Parke are prominent among those who have made important contributions to our knowledge of the life-history and ecology of some of the Phaeophyceae concerned in alginate production. Speakman is the leader of research on alginate yarns, while other aspects of the problem are dealt with at the laboratory of the Scottish Seaweed Research Association and elsewhere.

So far, the story of the utilisation of the larger algae has been centred in the western world, but I must now turn to some aspects of the uses of these plants in which the focus shifts to the oriental peoples. Although the Chinese and Japanese have used seaweeds in a great variety of ways, in agriculture, horticulture, medicine and pharmacy, in making adhesives and plaster boards, inorganic substances and alginates, it is as human food that marine plants have been most important in oriental economics. As I have mentioned, the earliest records of the use of seaweeds as food are in Chinese literature, and the Chinese still consume large quantities of the algae, but the Japanese have probably progressed furthest in the large scale production and harvesting of edible marine plants. In both countries the harvesters appear to be able to recognise small specific algal differences. In Japan a great effort has been made to increase seaweed production in the last century, and for a report on the present position I am indebted to Ferguson Wood of New South Wales who recently visited the Japanese islands. As large-scale fishing in Japan is by means of traps and set nets, there is no disadvantage in laying rocks or bamboo branches in in-shore waters for the culture of the algae. Members of all the groups are eaten, the Phaeophyceae and Rhodophyceae being most important. The plants are not used merely as appetisers, like dulse (*Rhodymenia palmata*) and Murlins (*Alaria esculenta*) in this country, but they form part of the staple food of the population. Of the Phaeophyceae the smaller species are dried, and Laminaria, Eisenia and Ecklonia are used in every household as noodles, toasted, served with rice or in soup. In addition, these algae are now ground and added to the flour in many prefectures, their use being sometimes obligatory. After harvesting, by grapnels from boats, the plants are partially dried, stacked in thatched ricks to allow partial fermentation, and then dried again. This mode of preparation may well be of considerable importance because of the possibility that it breaks down the algal proteins and carbohydrates into a form in which they can be digested by human beings. Other explanations must not of course be precluded, including the suggestion made by Ferguson Wood that the individual Japanese may develop an intestinal flora which is capable of dealing with algal material, fed, as he is, on seaweed from infancy.

Of the Rhodophyceae which are harvested for food in Japan and China, the genus Porphyra is of some importance. *P. tenera* is intensely cultivated on bamboo branches laid down in the littoral zone in many prefectures. After being harvested the plants are chopped, washed and dried into thin papery sheets, the resulting ' nori ' being used in a variety of ways and especially for wrapping rice balls. *Porphyra laciniata* is one of the few seaweeds which is extensively used for food in Britain. Sold as ' laverbread,' it is an important feature of the markets of the industrial area of South Wales. It is imported from Scotland and Ireland and gathered locally, particularly from the sandstone reefs of the Pembrokeshire coast. Each picker strides across the sands at low-tide, bag or bucket on arm, sack tied behind in a waist belt, and proceeds to gather the laver with a quick crisp plucking sound, like that made by cattle eagerly grazing in a lush meadow. Later, heavily laden, full sack on the shoulder, these women return across the shore, their rosy, broad, wind-tanned faces giving a hint of their Flemish ancestry. The weed is either dried in their cottages or in huts of driftwood roofed with straw, on the shore. It is then despatched to be boiled for the market. It is gathered all the year round, though the winter weed which is scarce gives the best economic return. The gradual elucidation by several scientific workers of the life-history of this plant, with its seasonal growth-forms which have borne different names, is an example of the botanical verification of a fact which was appreciated earlier in a cruder form by those intimately connected with the harvest of the plant. They realised that it persisted as a narrow ribbon-like form during the winter when the price was high and changed to broad fronds in spring, when the price fell.

Agar was the most profitable seaweed product in the Japanese export trade before 1939. Although a little was made in America, Japan held almost a world monopoly until that time. Agar can be made from a number of red seaweeds, but Gelidium species were considered

to produce the highest grade product. In the plant, agar, which is a complex polysaccharide, is a cell wall-forming substance whose quantity varies seasonally. Its exact composition is a matter of controversy but its chief component is a complex carbohydrate which may be combined with calcium and magnesium as an organic sulphate. Although originally made for food, and very widely used in the food industry, especially in ice-cream and canning, agar has come to be indispensable in the textile and leather trades, in making high-grade adhesives, photographic films and shatterproof glass, in medicine and pharmacy and as a medium on which fungi and bacteria can be grown. It is an essential product in all scientific laboratories for culture work, and during the second world war it was imperative that all countries should have a sufficient supply for the preparation of protective vaccines both for the civilian population and for the armed forces. The need was met in different countries in a variety of ways, from the production of a high-grade agar to the boiling up of unidentified red seaweeds which happened to be locally available. The U.S.A. enlarged its existing industry, and South Africa, India, Russia, Malaya, Ceylon, Portugal, Spain, Italy and Eire were among the countries which made small quantities of agar. China began an industry but the area was subsequently cut off by the Japanese. New Zealand, Australia, and I think I may add, Britain, set to work on the botanical aspects of the agar problem as well as on the making of a high-grade product. In New Zealand two species of Pterocladia were used and in Australia *Gracilaria confervoides* was the source of an excellent agar. It will be recollected that in Britain the position became much more acute after the incident at Pearl Harbour. Owing to Government foresight agar had been stored, but it was essential to see that supplies could be maintained if and when the stock became exhausted. Research was carried out at the request of the Ministries of Supply and Health. It was evident that of the seaweeds from which a good agar was most likely to be made, there were two that occurred in harvestable quantity. These plants, *Chondus crispus* and *Gigartina stellata*, were known collectively as Carrageen and had been harvested previously in Canada, Eire and France on a commercial scale, for direct use in the food, drug and other industries as well as for the extraction of a gel known as Carrageenin. This latter is a stabilising agent for drinks such as chocolate-milk and is also much used in the food, textile and leather trades as well as in medicine and pharmacy. Unlike agar, it forms a soft viscous gel. Around the coast of Britain these two plants were habitually harvested in small quantities by the housewives of the west coast of Scotland and the Hebridean islands and stored for winter use. Orr and Marshall, working at Millport, found that though the untreated extract of these plants was too soft to be used for bacteriological work, it could be converted into an agar by heating in an autoclave with the addition of potassium hydroxide, the whole being subsequently neutralised with hydrochloric acid. The resulting agar was rendered colourless by treatment with activated charcoal and was then entirely satisfactory for laboratory use.

The next stage in the solution of the problem was botanical. I was asked to ascertain where the seaweeds grew in harvestable quantity and to consider the consequent problems. I shall always be grateful to the scientists of many academic persuasions and universities, who obtained leave or sacrificed vacations and helped to survey all the likely areas of the accessible coasts around Britain. The work was hard but our compensations were great. North of the Firth of Lorne on the west coast of Scotland was one of the areas for which I was personally responsible ; the mainland coast yielded little carrageen but some of the islands were very rich, the estimation for Skye alone being 146 tons. Of the scenic beauty and fascinating natural history that surrounded us on our expeditions to the Hebrides, I cannot speak here, but I hope I shall never lose the memory of the raised beach that forms the Treshnish Isles, the Dutchman's Cap and Lunga, rising from a rose-coloured glassy sea in the sunset, their soft blue mountains standing like ethereal castles above the old water-level.

As a result of the surveys it was found that harvesting would be justified in a number of areas on the west coast of Britain and in Northumberland. Investigations of the autecology of the plants were undertaken by a team of workers in Aberystwyth, Glasgow and Manchester, and experiments were conducted to find out the best methods and times of harvesting having regard to the growth and fructification, the maturation and regeneration of the plants and the seasonal fluctuations in the gel strength of the extract. When it was found that not more than 5 per cent. of the total harvest would be Chondrus, experimental work was confined largely to Gigartina.

Urgency dictated the order of our investigations, and decisions regarding the harvest had to be made on the basis of ecological results as we obtained them, without waiting for full knowedge of the life-history of Gigartina. Kathleen Drew, at Manchester, is still investigating the cytology of the plant but recent results by H. T. Powell, at Millport,

where spores have been grown on sterilised sandstone blocks in the sea, lead one to believe that the so-called carpospores grow directly into new Gigartina plants, that is, there appears to be no separate tetrasporic individual—a possibility much discussed hitherto.

The actual harvest was our next problem. The amount we allowed to be harvested increased with our knowledge of the regeneration of the plants. In the years 1943–45 over 300 tons of wet weed were gathered, mostly by people who had no previous knowledge of seaweeds or of the shore. The plants are small and they grow on rocky boulders near low-water mark ; in most places Gigartina, which formed 95 per cent. of the crop, can only be gathered for two hours before and after low-water, on about four days in each fortnight, so I think this quantity is a great tribute to the lay-workers. Scouts, Guides, Sea-scouts, Rangers, Junior Red Cross, W.V.S., W.R.I.'s, Lighthouse-keepers, the R.A.F., Schools, private individuals and the County Herb Committees, all helped us. Algologists, who were responsible in the areas for teaching the uninitiated to identify such a polymorphic plant as Chondrus, had their own problems—mercifully we did not need to collect Chondrus and Gigartina separately. I shall never forget the enthusiasm of a group of little girls, evacuated from the Liverpool dock area, whose excitement increased with their wetness as they delved into rock-pools and scrambled over seaweed to know if their finds were ' right, Miss.'

Since the war, harvesting has continued on a commercial basis and an excellent agar is being made by Fine Industrial Commodities Ltd. It is suitable for all the purposes for which Japanese agar could be used, including bacteriological work, and is actually much more transparent than the Japanese product.

In addition to the uses of the seaweeds which I have outlined, there are many of lesser importance which enrich the interest of the literature. In an early Welsh text the algae are referred to as possessing magic power. The larger brown algae have been used as fuel on many islands where coal is difficult to obtain, and have served to till the ground in St. Kilda, tied to the teeth of the harrow. Laminaria stipes have made effective knife-handles and walking-sticks ; Chorda has been made into whips. The Phaeophyceae are used as Christmas decorations in Japan, and a seaweed extract stretched over gauze frames has been used for making Chinese lanterns. During the war I was asked to help with the preparation of dried Laminaria stipes that were to be inserted, dry, into wounds which needed to be kept open. Ceramium is said to have been used by Hebridean maidens to enhance the colour of their cheeks, though I suspect their beauty was more likely acquired for the same reason that ' dabbling in the dew, makes the milkmaids fair.'

Landsborough in his treatise on the seaweeds observes that the book of nature is like the book of grace ; the wonders they contain must be ' sought out.' I said at the beginning that I hoped this brief review of algal utilisation might enable us to look ahead and see the lines along which further work might most profitably be directed. I hope that it may also help us to consider the future potentialities of these plants. Contact with the practical arrangements for the world production of agar has forced upon me certain general considerations on the need for co-operation not only among research-workers in many branches of science but between the scientists and the commercial world, in the economic utilisation of the seaweeds as a whole. In these studies, the facilities offered by marine biological stations and other academic institutions on the coast are obviously very important. Naturally, I shall draw my conclusions mainly from the work with which I have been most closely associated, but I believe that many of the findings will be in accord with those of my colleagues interested in other modes of seaweed utilisation.

The marine algae live in the constantly changing medium of seawater. Their distribution and growth is bound to be dependent on the temperature, oxygenation and chemical composition of that medium, so that the work of the oceanographer is one of the fundamentals on which the study of the plants must be based. Consideration must also be given to the effects of pollution on marine plants. Many of the algae are attached to rocks of varying structure and chemical composition. Though they do not derive food from the substratum, its suitability for the initial lodgment of the spore and its ability to withstand the inevitable pull exerted by the adult plant, buffeted by heavy wave action, must be of primary importance. In view, however, of the extreme sensitivity of germinating spores to minute traces of certain metals and of our observations during the coastal survey of the sudden absence of algae on a stretch of shore where geological change has not removed suitable crevices, I wonder whether the substratum is not capable of affecting growth. Can the germinating spore, for example, be influenced by substances gradually leached out from the weathering surfaces of the rock to which it has primarily adhered ? This is one of the ways in which the botanist and the geologist

need to co-operate for the satisfactory solution of a widespread problem—a problem which, incidentally, is linked with the possibility of the production of anti-fouling paint and the rendering of ships capable of greater speed because they are clean below the water-line.

The first principle governing the study of the plants themselves must obviously be systematic; in the Rhodophyceae the ideal system of classification has not yet been found. I must also stress the value of the long-term intensive study of a single species. The marine algae are extremely polymorphic, and in literature that has not recently been checked, there is undoubted confusion in many instances between true varieties and growth-forms. This was apparent in our study of both Chondrus and Gigartina. For the elucidation of this problem the plant needs to be studied not only in all its environmental conditions at all seasons but there is urgent need for more cytological work too. The nuclei are small and difficult to handle, but modern cytological technique has advanced rapidly and I hope that in the not distant future more cytologists will include the algae in their investigations, not only in regard to the position of reduction division in the life-cycle, but also in the unravelling of their complex systematics.

Systematics lead directly into two other branches of investigation—the one physiological, the other ecological. Plants of *Chondrus crispus* could hardly be more different from each other than when growing in the littoral and sub-littoral zones. These two habitats are subject to the influence of diverse ecological factors—the littoral plants exposed at nearly every tide, often to the full glare of the noonday sun. How do their physiological processes compare with those of the sub-littoral plants that are rarely or never uncovered and always receive their light through a seawater filter of varying depth? This is but one of the numerous problems of growth and nutrition which await solution. There is also need for further investigation of the cell wall-forming materials and the products of photosynthesis. At present it appears that the human intestine finds the algal proteins and carbohydrates very difficult to digest. The Japanese, by some means as yet undetermined, seem to have succeeded in obtaining more nourishment from an algal diet than westerners can absorb. It is clear that a large field awaits the bio-chemist here. In the utilisation of algal material as such the high mineral content is a disadvantage, but if the extracted carbohydrates could be rendered capable of easy assimilation by humans, our food supply could be greatly enriched and quantitatively increased. The vitamin content of algal tissue offers another promising field of investigation. The presence of growth-promoting substances was suspected before the vitamin content as such attracted the attention of research workers. The work of Tilden, Lunde, Lie, Heilbron and others leads one to think that a still wider vista lies ahead.

That the algae are capable of selective absorption of salts from the sea-water, as is shown by their high iodine content, is another phenomenon that suggests new fields of study. The presence of trace elements greatly influences the growth of these plants in culture, but much more investigation of them is necessary to ascertain which elements are of most importance for the various groups of algae. In connection with the accumulation of substances in the plant which are scarce in sea-water, a new problem has clearly arisen because of the need for disposal of radio-active waste. If radio-active elements, in quantity, were to be put into the sea and taken in by the algae, new problems might arise which needed the close co-operation of the chemist and botanist with the manufacturer of seaweed products for human consumption. Whether such substances entered the plant by ionic exchange or adsorption, their presence might be an embarrassment in the utilisation of seaweed extracts in the food industry, and their effect on the fauna which browse on the seaweed would also need careful investigation.

Ecological study is inevitably strongly linked with the needs of commerce and with the training of the harvester. In addition to the location of the plants and the determination of harvestable areas, detailed study by means of quadrats and transects, carried out in different localities and over several seasons, provides a good deal of information concerning the habitat and growth of the plant, its fruiting periods, longevity, modes of regeneration and perennation. The relative importance of these different ecological aspects will vary, inevitably, with different commercial requirements. In the case of Chondrus and Gigartina ecological study enabled us to determine when and how the harvest might safely be taken, without endangering future crops. The plants must obviously be gathered when the majority of them are at a maximum development in regard to the product required. Gigartina increases the number of cortical rows and the thickness of the walls as the summer advances and the gel strength of the extract increases accordingly. When growth and gel strength are at a maximum, fruiting begins, and the harvester has not only to gather the crop in the appropriate season but has to be careful that the future supply is not permanently impaired. The period or periods of sporing need especially careful investigation because,

as in Chondrus, the tetraspores may be borne in winter and the cystocarps in summer. Such a phenomenon may confuse the results on the seasonal periodicity of gel strength. There may also be a difference in fruiting between plants from one locality and another or from one part of a shore and another. How much the position of a plant in relation to low-water mark may affect its fruiting period has been admirably demonstrated recently by Parke's work on *Laminaria saccharina*. When the harvest is gathered, some sporing material must obviously be left and the plants which are gathered may have to be plucked in a particular way, in order that regeneration and perennation can take place. Chondrus and Gigartina regenerate from broken thalli, provided the break is not too low down but *shearing* seriously affects future harvests. In addition, they perennate by means of the hapteron, an important organ of which the form and function need careful consideration. The hapteron of Chondrus has a multicellular attachment layer at the base from which the upper food storage region is easily torn away. The plant needs therefore to be picked with the greatest care or the parts capable of regeneration are removed entirely. This tearing away occurs frequently in nature and causes a plant which can be a perennial to behave as an annual. In some localities where shingle grinds the algae in autumn storms, a very large proportion of the population of Chondrus plants behave as annuals. In the case of Gigartina many of the plants in a population are likely to be several years old ; the hapteron has no sharply demarcated layers for attachment and storage ; it is consequently much harder to remove from the rocks and can be picked with less risk of permanent damage.

Another aspect of algal study is the interrelationship of the plants and animals on the shore. Some pioneer work has been done by Colman, Stephenson and others, but a wide field is still unworked. This inter-action of biotic factors may be of the greatest importance, as in the case of *Gigartina stellata* and other algae which can be removed completely from a boulder, in a zone which otherwise has optimum conditions for their growth, by the browsing of limpets. Alternatively, the large-scale harvesting of algae from a locality will remove the shelter which they afforded for some animals and will open crevices for others which formerly had no place in them. Such interference with the balance of life in in-shore waters may always have far-reaching consequences, and there is especial need for co-operation between botanists and zoologists in the investigation of this problem, especially when the large-scale harvest of the Phaeophyceae is involved.

Another field in which little has yet been done is the study of the part played by fungi and bacteria in the general economy of the sea and its living organisms. I think that ultimately the fungi will prove to play a larger part in seasonal decay than is yet appreciated. Here again, I can illustrate my point with an example from Chondrus. I. M. Wilson identified *Leptosphaera chondri* on the fruiting pustules of the host. Subsequent observation has revealed that the fungus occurs on both tetrasporic and cystocarpic pustules. I strongly suspect that it plays a very important part in the breaking down of the tissue which releases both carpospores and tetraspores ; hitherto these have been said to be released only on ' the decay of the thallus,' a phrase with which I am afraid we cloaked our ignorance.

When the intensive study of individual species has revealed facts that lay the foundation for commercial enterprise, the need for a greater tonnage of raw material is likely to be the next demand. Where fisheries do not need to be considered, some progress with large-scale algal culture may be practicable, but there are very serious restrictions to its use. Few plants are as accommodating as Porphyra which grows freely on sticks put into the sea in the right zone, or as the Fuci which readily colonise large stones laid in tidy rows on suitable shores. Many algae, of which Gigartina is an example, cannot become primary colonisers, partly because they are intolerant of the desiccation that is necessarily involved if individual young plants are exposed during low-water in the littoral belt. Some progress has been made in Eire by the experimental removal of the competitors of Carrageen but our results on weeding led us to the conclusion that for Gigartina and Chondrus it was not worth the labour involved. The artificial culture of plants in geographical locations where they do not already occur, has been considered by some workers, but from what I have said already, it is clear that such interference with the living organisms of in-shore waters can have far-reaching and irrevocable results, so that such action must be contemplated with extreme caution.

The economic aspects of the harvesting problem cannot be neglected and it is evident that the gathering of large quantities of wet seaweed from littoral or sub-littoral regions presents its own especial difficulties. It has been achieved successfully in Japan by the availability of very cheap labour and by the employment of women and girls as divers in the relatively shallow water. Harvesting in Japan is also helped by local conditions which make the culture of some of the plants much more practicable than it is elsewhere.

The nature of the substratum on which the crop grows and the relation of the plants to low-water mark are also important economic factors. In Australia, for example, where *Gracilaria confervoides* is used for making agar, the plants grow on sand-covered rocks between the tide-marks and are much more easily gathered in bulk than small weeds like Carrageen which have to be hand-picked by individuals, climbing over boulders. The bulk harvesting of the deep-water brown algae has its own difficulties and calls for special engineering skill. For example, where large masses of Macrocystis float by means of air-bladders embedded in their thalli, even though they are attached to the substratum by very long stipes, they are easily located. Mechanical means can then be used for large-scale harvesting and the economic position is correspondingly easier than in the case of Laminaria, which, may be not only submerged but attached by relatively short stipes to the rocky sea-floor.

After harvesting, the storing of the weed needs careful study if it is to remain in good condition.

It is evident that all these considerations, scientific as well as economic, must be taken into account in assessing the world's algal resources, but I hope that this summary has emphasised the fact that research in pure science as well as in applied must be pursued with all our vigour and enthusiasm, if the maximum use is to be made of the vast potential resources that are being rhythmically and unceasingly produced in the waters that surround our shores, and indeed in the surface layers of the oceans too.

Attention is being focused constantly on world food production and it is, in fact, the main theme of this meeting of the Association at Newcastle. If, as Osborn states in his book *Our Plundered Planet*, it is true that 'the tide of the earth's population is rising and the reservoir of the earth's living resources is falling,' it would appear to be of particular importance to make the maximum use of the resources of the sea. In so doing I think we should try to profit by the mistakes of those who first exploited the resources of the land. When large-scale seaweed harvesting has upset the balance of life in in-shore waters it may be too late to seek a remedy, and at present the integration of specialist knowledge in many fields is essential. At the same time there is no escape for the botanist. When the surveyor has located the weedbeds by eye, dredge, aerial camera, or echo-sounder, when the engineer has devised the mechanical means of harvesting where such is desirable, and the economist has assessed the cost, the manufacturer can follow the work of the chemist and market the product, but it must *all* be based on the sure foundation of pure botany on which alone can the superstructure stand effectively.

SECTION L.—EDUCATION

THE WIDENING SCOPE OF THE STUDY OF EDUCATION

ADDRESS BY
SIR FRED CLARKE
PRESIDENT OF THE SECTION

IN this address I follow the salutary practice of my predecessors in this chair and choose a topic that falls properly within my field of interest. We might fairly begin by asking, 'Why study education at all?' The question is still put with sceptical implication, even in the England of to-day. A fair answer might have been given in almost any age. But not only would it differ in some respects from the answer we have to give to-day; our contemporary answer would also have to come with greater force and urgency. I should put the point briefly by saying that the wheel has come full circle and that the study of education has returned to where it was placed by Plato and Aristotle, at the very centre of the study of politics, giving to this latter term the wide meaning it had for the Greeks.

That this has happened may not, as yet, be very widely recognised in England. But it will have to be. However great may be the differences in detail between our situation and that of the Greeks, their thought about education has quite peculiar relevance for us to-day. Education has become a major instrument of policy, say, if you like, a main agency of social control, and that in a nation which daily grows more concerned about its peculiar duty, both to itself and to the world.

It is becoming possible to see the reason why this should be so. There is no resisting the social and political effects of the coming of a highly complex technological society. I need not here go into detail concerning these effects; note is being taken of them on every hand, and every branch of policy has to take account of them. What we have to emphasise for our present purpose is that whatever else such a society may neglect, it cannot neglect the education of its people. Upon that it must, at all costs, keep its supervising eye and its organising hand, with all the risks to freedom and a generous humanity that may involve. We know well that the purposes to which education is directed, and especially the spirit in which, and the methods by which it is conducted, will differ greatly from one country to another. But all alike have now to take the education of their peoples with an almost deadly seriousness. Some will say that we in England have begun to do so only at the eleventh hour, and some will argue that we have not begun even yet. The Act of 1944 is at any rate evidence of declared intention. Moreover, and this is important, the Act does recognise amply that formal 'schooling' is only a part of the total influences which a society by deliberate action may bring to bear upon the education of its members.

Some light may be thrown upon our contemporary situation if we take a glance backward over the two centuries or so that preceded it. In doing so, let us remember how precisely educational thought and practice reflect the life of their time, the ideas by which it is animated and the standards by which its conduct is guided. If we do this, we shall be struck, I think, by two phenomena. First, that until quite recent times, education was, in content and method, very largely a matter of *tradition*, and second, that the golden age of liberalism was also the age of maximum security. We miss much of the significance of the nineteenth century if we fail to take full account of this latter collocation.

First, as to the traditional character of education. For the greater part of this period, systematic schooling was still selective, touching only part of the population. And when compulsory elementary education was enacted—within the lifetime of the elder of our contemporaries, be it noted—the traditional character of education still persisted. Let those who can remember the Victorian elementary school testify.

What were the governing ideas of this regime of tradition, or at any rate its implications (since it was not the English habit to clarify ideas)? We may say briefly that the ruling notion was that of an authoratitive body of knowledge and an accepted code of belief and conduct to be communicated. Moreover, the communication was to be effected by the traditional methods. We know well what these were. Dr. Johnson's

opinion on the matter of applying study to these questions is well known : ' Sir, it is no matter what you teach them first, any more than which leg you should put into your breeches first. Sir, you may stand disputing which is best to put in first, but in the meantime your breech is bare. Sir, while you are considering which of two things you should teach your child first, another boy has learnt them both.'

Of course there were variants and breakaways even in the eighteenth century. It would seem that the experimenting and innovating academies of that age were even more numerous and widespread than had been suspected. Still, it remains true that the experimenting and innovating spirit was active only outside what may be called the ' official ' system. Thus Halévy, in his monumental ' History,' points out that the universities had no part in the great scientific discoveries that marked the later eighteenth and earlier nineteenth centuries.

The conclusion is the same if we look at that type of education which was provided, not by the school but by the kind of culture in which the individual was reared. This would include the education of the illiterate. Here again the culture was traditional, a thing of time-honoured use and wont. Some of us can recall fragments of it from our young days in the forms that shaped the ideas, the habits and the diversions of the common people.

The change brought by nineteenth century liberalism in all this was less than might have been anticipated. It did bring at least two outstanding contributions. One was science, the other was the idea of the autonomous, self-sufficing individual. Science imposed itself in two ways. First was the claim of scientific studies for a place in the curriculum. And let us note that when the claim was made good, these studies, too, tended to fall under the sway of tradition ; formulated ' schoolstuff ' to be learned, rather than a spirit to be caught and applied.

The other form of the impact of Science was, perhaps, more fruitful. It consisted in the search for *rational* grounds and principles for the guidance of practice as against the dictates of tradition. So we get that application of biological and psychological studies to the understanding of education which has been so fruitful and in many ways so beneficial.

Animating these studies was a conception of the individual that was of the essence of nineteenth century liberalism. It was a conception, thoroughly and, it would seem, incorrigibly *abstract*. The individual was understood formally rather than concretely in the cultural setting that gave him his specific characteristics. He ' developed ' rather than ' learned ' ; teaching was the application of stimuli rather than training in quite concrete disciplines. In its extremer forms the conception looked upon ' development ' as so sacred a thing and so much the result of impulsion from within that one would think that education in any serious sense was worse than a superfluity—it was a criminal and sacrilegious interference. Fortunately the exigencies of practice could be trusted to correct the wilder extravagances of such theorizing.

While it is true that this typical nineteenth century doctrine with its growing train of scientific interpretation did something for the humanizing and enlightenment of teaching methods, it yet left a whole wide area of the study of education largely untouched.

In this area tradition still held most of its unquestioned sway. This was the region of those sociological and common cultural factors which, as we now realise, quite apart from schooling, have such potent influence for good or ill on the young. There was some study of the history of education but it consisted either of accounts of educational thinkers or of compilations of facts about schools. The whole great drama of changing English society and economic revolution found little or no place in it. For the rest, evolutionary theory had far more influence than any study of the bearings upon education of the revolutionary social changes of the nineteenth century. Sociological and cultural factors were simply taken for granted and what I should call, in the Greek sense, the ' politics ' of education, had, after Matthew Arnold, hardly any outstanding exponent till we come to Michael Sadler.

What can account for this strange neglect, now being remedied with some risk of overemphasis in the opposite direction? I suggest that the nineteenth century notion of the discrete and autonomous individual is not a sufficient explanation. The phenomenon, like so many others that have been too little noted, was, I am convinced, directly related to English *Security*. Things *seemed* like that in the relative peace and prosperity of that expanding and energetic age. But could we possibly think like that amid the turmoils, the upheavals and the menaces of this century? Painfully enough we are realizing that we can't.

The effect of this nineteenth century inheritance has been to enshroud education as the ordinary man conceives it in a certain unreality, something not characterized by any particular urgency such as marks the things we fight over in politics. It is ' A.B.C. stuff ' or a necessary ritual to which the young have to submit, or a rather mysterious business of scientific or pseudo-scientific speculation

and study about which 'educational experts' talk in unintelligible language. Of course, it is, in fact, nothing of the kind, it is now the urgent business of everybody, the only admissible means of control of a free people, the mode of planned and universal social action upon the results of which destiny may well turn.

Such a change of vision necessitates just that enlargement of the scope of the study of education which is the subject of this address. But how has that change come about?

Any attempt to answer that must take account of the caution to which we are now accustomed in handling problems of historical causation. Nevertheless, it is possible to indicate some at least of the influences which have brought about the change.

First we may note the effects of that new vista of opportunity, which, in the nineteenth century, opened up before the eyes of the more enterprising of the working classes. The scholarly re-writing of the history of the Industrial Revolution which we owe to recent work, while it leaves standing much of the record of working-class hardship and resentment, redresses the balance by emphasizing the gains, actual and potential, which accrued to the workers. The standard of life rose; new instruments of action came into play, and workers shared in that general mood of expansion and optimism that so characterized this period.

Particularly it must be noted that they were now more concentrated in the cities and towns, a factor facilitating collective action, and making for more effective leadership. So through trade union, chapel congregation, co-operative society, and in similar ways, they strove and learned. Especially did they *learn*, and it is much to be hoped that the historians of education will work upon this fruitful field of out-of-school adult learning.

So we get this sense of new possibilities to be striven for and achieved, which, in time, came to provide a powerful driving force towards increased educational opportunity. True, the motive was often just the very natural one of 'getting on' in life. But the finer motive of increasing the worth and dignity of living was by no means absent either. While it must be admitted that such motives were never universally active—compulsion had to be imposed and was often resented—it remains true that a strong popular urge was a potent factor in bringing about the change we are considering.

Another was a decline in the general sense of security and a tempering of the earlier buoyant optimism. It was not until the end of the century that national security in the military sense came to be a source of anxiety. But long before that, uneasiness concerning commercial, industrial and cultural health found increasingly anxious expression. The voice of Matthew Arnold was only one of the more eloquent.

Such a motive was calculated to influence the governing rather than the governed classes. It is, perhaps, a fortunate thing that this upper class anxiety should have coincided with the popular urge we have described. The coincidence made for stability in a time of accelerating change and helped to produce that crop of social legislation, much of it put through significantly enough, by Conservative Governments, which we witness in the later part of the century. At the same time, we note the rise of a positive, constructive Liberalism and the beginnings of the Labour Party.

Behind all this were the irresistible and accumulating forces that were generated by the maturing processes of a new kind of society, the large-scale technological society, now more fully developed in our own time. Such a society cried out for appropriate measures of organisation and control. Its inherent tendencies were towards autocracy, possibly irresponsible, and a progressive dehumanizing. The two developments, that of a democratic urge, and that of technological advance, were by no means on all fours. So to-day we are in the midst of controversies concerning the methods by which these can be reconciled, in brief, how we can be *efficient* in the modern sense while still remaining a free people.

A whole crop of consequences follows: the change in class structure; the shift and wider diffusion of political power; the powerful organisation of economic 'interests,' often at odds among themselves; the problems and risks of a so-called 'mass' society; the struggle to maintain traditional *values* by working out the new forms in which they can be embodied, and by discovering and cultivating those members of our society who can best express and safeguard them.

At all this I can only glance. What I would wish to emphasize is that we have now reached a stage where the qualities developed in the common citizen are of crucial importance, and we are also reaching the perception that in principle, *all* the resources of society, and not the school alone, are involved in the ensuring of these qualities. Both these realizations find expression in the Education Act of 1944, and even more strongly in the ramifying of day to day administration.

Thus we are committed, in principle, to what I have called the Educative Society, a society so placed that it must recognise that the qualities developed by its citizens are a first concern, that in modern circumstances the development of such qualities necessitates

universal and somewhat prolonged education, and that, in principle, none of the resources of society may be regarded as irrelevant to this purpose.

Before I go on, however, to outline the enlarged scope that this implies for the study of education, I would add a word concerning the studies that have been most vigorously pursued hitherto. Apart from much useful work on the techniques of teaching method and administration, these consist principally of studies, growing more and more scientific in inspiration and procedure, in the biological and psychological foundations of educational practice. Now I want to suggest that the impulse toward such work did not come unsolicited out of the blue. Nor did it spring from a purely scientific impulsion as though to say: 'Go to, now; we have become scientific over many things, let us also be scientific about education.' The driving motive was more pragmatic than that. It had its main scource, I am convinced, in the necessities of a very real practical situation, and that, I am equally convinced, was the need to transform the elementary school. It is to be noted that the schools we now call 'independent' were little concerned in it, if at all. We have seen that, almost until the end of the century, the elementary school was tradition-ridden. There was a defined and authoritative, if meagre, body of knowledge and skill to be transmitted, and this was effected by the traditional school methods. There are those among us who can still remember what 'Method' and 'School Management' meant in those days. But with the turn of the century came a striking change, and the liberalizing of the elementary school is a great and stirring theme of the succeeding years. The studies in biology, psychology and teaching method were intimately related to that transformation.

To-day we stand at another critical turning-point. Retaining the focus of interest as we have it in the needs and characteristics of children as we find them in the primary schools of the new regime, we have now to explore what comes into view with the new focus in the characteristics of adolescence. What the elementary school was for the studies of the first half-century, the secondary school and the county college will be for the expanded range we shall have to explore in the second half. Sociology and social psychology; history of education not just a tale about schools and thinkers, but as an aspect of social history; comparative education; and above all, philosophy, will have to be provided for and pursued. Nor would I admit that even theological studies are wholly irrelevant to the practical task.

Here you will permit me, I trust, to illustrate the theme by reference to an effort of organisation of which I have some knowledge. It is the scheme of post-war development that was prepared for the University of London Institute of Education. This plan contemplated a minimum of *five* essential professorships, each to be concerned with one of the main lines of approach to the study of education. These five are: philosophy, psychology (including social psychology) sociology, history and comparative education. Others, of less basic significance, may be added as the need arises and opportunity serves. It is good to know that all five chairs are now established.

A few comments on this scheme will be in place. If there is to be any primacy among the five from the point of view of the student I should place it with philosophy. I trust the day will soon come when no-one is admitted to higher studies for a degree in Education unless he can show evidence of having undergone a sufficient discipline in philosophy. The consequences of a lack of it can be too serious. Psychology is now a well-established study with some distinguished representatives, and the kind of psychology that is relevant to education is now better understood. Sociology presents difficulties. There is still, in my opinion, insufficient recognition of the place of education in the general study of it. I have seen detailed syllabuses for undergraduates which did not so much as mention the word. But enthusiastic work by ill-prepared amateurs can do much harm; the subject has its own discipline and must be handled by a master. The proper content of it for the student of education has still to be satisfactorily worked out.

For the history of education we have at long last a Chair expressly devoted to the study at King's College in the University of London. There is no doubt that the study will be promoted in its proper context, so long neglected, that of social and economic history.

Lastly, for a country with obligations such as ours carries, provision for comparative education has high and growing importance. But the just interpretation of the phenomena requires such a fund of varied knowledge that this study can be profitably pursued only by relatively mature students. Its relation to the existence and work of U.N.E.S.C.O. will not be missed.

Such, in broad outlines, is the basis of the London plan. We may expect that, as University Institutes of Education establish themselves throughout the country, appropriate variants of it will take shape at each centre. We shall then be able to claim that we are reasonably well-equipped for the study of the problems with which the expansion of education in range and direction will now present us.

Certain consequences of such a development have to be noted. In the first place, most of the studies I have enumerated are also provided for in the general courses of the University. Should we not hope for a fruitful partnership to grow up between what we may call the ' pure ' and the ' applied ' studies? I have long been casting an envious eye on great resources that are present in certain Universities, well-used as they are, and hoping to see the day when they could be brought into more fruitful relation with the studies we are here considering. Not many of such resources are wholly lacking in relevance for education. Then what a prospect that may open out for the ordinary student-in-training. The enlarged conception of his job that he may reach is only one of the possible gains. Doctors and dentists can be effectively trained only where medical studies are being actively pursued. Is not much the same true of the training of teachers? With the coming of University Institutes of Education I should look for a considerable increase of sheer intellectual discipline in the training-course itself, and that alone will be a great gain.

Finally, the advantages to be derived from all this range of studies should not be confined to those who are professionally engaged in education. We are still suffering in this country, though less than formerly, from lack of a sufficient body of well-informed public opinion on education. True, some parts of present-day work in education are highly technical and bound to present something of mystery to the uninitiated. But if we are to be a genuine democracy and if education is to be vital and healthy, there is need for critical, if friendly, public opinion backed by a sufficiency of knowledge and understanding. Some at least of the studies I have enumerated, as their results are spread abroad, should contribute materially to this desirable end.

And now at the close of this long excursion, not too tedious I trust, we come back to the occasion and justification of it all ; boys and girls at school. How much of the fruits of what we have been considering will get through to them, and *how* will it get through to them? Something, a good deal, will depend upon the general health of our society. You cannot look for healthy schools in a sick society, nor can you blame schools for what society prevents them from doing, or speedily undoes what is actually achieved.

But at the critical point of the whole conception we must set the artist-teacher, the performer in what I should regard as the most difficult of all the arts, and perhaps the most rewarding. It is vain to hope that among a quarter of a million or so, all will be artists in the high sense of the word. But with a preparation that illuminates the full scope of the art and provides a disciplined understanding of its conditions, we may hope that the gifts of the artistically endowed will be fructified, and the more commonplace powers of the rest may be brought to ensure at least intelligent and competent craftsmanship. Steadily, if a little slowly, the English people seem to be arriving at a juster estimate of the high significance of the teacher's work and of the rights and the scope that must be accorded him if he is to discharge his function. Perhaps they are beginning to be even a little frightened at the possible consequences of not achieving and acting upon such an estimate.

However that may be, the vision that some of us have caught does seem to be within reach of fulfilment ; that of a profession enlightened, self-respecting and equipped with its own proper discipline, freely co-operating with those studies that the universities can provide for in full measure, and converting the results into skilful guidance and well-ordered spiritual nourishment for the nation's children.

If there is any ground for doubt or misgiving in our contemplation of this prospect, it is to be found in the risk, always present, that these studies may get out of relation with their practical focus, the wise education of children. They may acquire a detached interest of their own, become more and more speculative and theoretical, and so become just one more of the vested interests. This would be no new thing in history—sharp separation of concept from act, of theory from technique, is a persistent tendency in man's cultural record.

Nothing must be said here to depreciate the importance of continued provision for what we know as the ' pure ' studies. Their relation to the ' applied ' is vital. But it is applied studies that we have in mind here. For preserving that concentration upon their practical focus which is so essential, I should rely mainly upon the play of informed critical opinion coming strongly from the schools and colleges, but also from interested groups of members of the public. What will now be intolerable is continued acquiescence in a condition where educational practice goes one way and theory another, saluting one another only on formal occasions such as Speech Days. If that state of things should continue, we may find that in developing the wider range of studies of which I have been speaking, we have merely saddled ourselves with another ineffectual burden. It is devoutly to be hoped that that will not happen. Personally, I do not think it will ; there are factors and forces still strong in English life which may be relied upon to prevent it.

SECTION M—AGRICULTURE

FARMING, SCIENCE AND EDUCATION

ADDRESS BY

PROF. N. M. COMBER

PRESIDENT OF THE SECTION

IT is properly expected that an address to a Section of an Association for the Advancement of Science should be concerned with some current scientific developments in the subject of the Section. Were I to ignore any aspect of the influence of scientific study and research upon the farming industry I should indeed be proved unworthy of the honour of the Council's invitation to the presidency of Section M, an invitation which I accepted with some temerity after looking through the names of my distinguished predecessors. But I do want to try and put scientific agricultural developments in their perspective and to relate them to certain fundamental characteristics of farming that remain permanent and basic.

It is clear to everyone that during practically the whole of the history of civilised life farming has been practised as an art and a craft and has been learned by experience. It is indeed a little more than a century ago that the primary process of photosynthesis involved in the growth of a plant was discovered, and, as I will explain later, the fundamental science of crop husbandry is deeper than that and is still quite obscure. During the last century, however, from the time of the discovery of photosynthesis and the beginnings of the Rothamsted experiments and the formation of the Royal Agricultural Society, scientific knowledge about agricultural processes and materials has developed enormously and with the development of this acquisition of knowledge there has been a development of educational schemes to enable those concerned with the agricultural industry to study these scientific developments. There are, therefore, three groups of factors involved in carrying out a farming policy. There is first the art and craft of farming; second, scientific knowledge of the subject; and third, the education of those who can make use of this knowledge. Now the relative emphases on the parts played by these three groups of factors has changed very much during the last hundred years, and in examining the position of each of them at the present time I reach these conclusions :—

First, that among the majority of the people of this country there has been a grave and almost complete loss of appreciation of the status and prestige of farming as an art and a craft, and that this is a great danger to future social and economic life. Second, the application of science to the industry of farming, essential as it is in modern conditions of life, will never make farming an applied science in the same way that many other technical industries are: the art and craft of farming will always retain its fundamental place. The third conclusion confirms an opinion I expressed to a combined meeting of the Education and Agriculture Sections of the Association thirteen years ago at Blackpool, and I will quote these sentences from the paper I then read: '. . . unfortunately, the proper balance between the acquiring of new knowledge and the education of those who can use it on the farm has not been preserved . . . a century ago the most urgent call on agricultural educationists was the acquiring of knowledge . . . the most urgent need in agricultural education to-day is the education and training of the young people entering the industry.'

THE STATUS OF FARMING

The fact that farming is fundamental to physical human life is obvious enough to anyone who gives it a thought. But this fact has been increasingly neglected by more and more people during the development of an industrial age in which the vast majority have ceased to have any direct contact with farming. That has been the subject of many lamentations. In his Presidential Address to Section M in 1927, Mr. Morison pointed out that it had long been the complaint of people interested in agriculture that it is very difficult to arouse interest in the minds of those people not connected in any direct or obvious way with the industry. He asked if it was not time that some effort was made to inform the whole body of consumers, not only of the way in which their food is produced, but of the manner of life of those who produce it and the problems that affect the countryside as distinct from the town.

Mr. Morison raised that question over twenty years ago and I am not aware of anything substantial and successful that has been done in the matter since. But the conditions of another Great War and the real danger of starvation have brought home to everyone the fact that farming is a fundamental thing in human life, and it is worth while to re-examine the position after this war-time influence and experience. Has the influence of war-time brought about something of the general and acceptable regard for farming and the understanding of its problems for which the President of 1927 pleaded? I think not. Is it likely that the many assertions in war-time speeches that the nation would never again forget its farming will be fulfilled and be brought above the level of mere platitude? I doubt it very much unless something scarcely yet attempted is done in the matter.

But in order to understand the effect of war-time conditions it is necessary to consider the regard and lack of regard for the status of the farming industry in days of peace and serenity. In those happier days all people used the products of the agricultural industry as tokens of the highest values in human life and relationship, but for the most part they failed to interpret the token and diagnose the true status of farming, although this status is betokened by the unique way in which its products are used differentiating it from every other industry. At all times and in all ages people have sat down to eat together in token of goodwill and cordial relationships, and, conversely, there is no more bitter insult to be given to a man than refusal to eat with him. Farming provides more than the mere physical necessities of life. It provides the symbols of the highest values in human life. We do not invite our friends to dinner because we have taken pity upon their state of malnutrition; a student of the science of nutrition will, it is to be hoped, forget about his science when we invite him to dine and it is still more to be hoped that he will forget about it when he invites us to dine with him! Nobody assesses the value of a wedding banquet with the aid of a bomb calorimeter, and the menu of the annual dinner of Section M of this Association is not based on the calculation of starch equivalents! The way in which men use the products of the soil in normal and peaceful times belongs primarily to the art of living which is far greater than the science of living. So it is that the products of farming are on a higher and different kind of level from that of the products of other industries.

Now in pre-war days the majority of people who used their food in this happy and healthy way never thought back to the reason for it all and to what formed the unique status of the farming industry. Why is it that farming provides not only the necessities of physical life but the tokens of the highest spiritual value in human life and human relationship? This high status of farming lies in the fact that it is inextricably connected with the status of human life itself and in a fundamental way in which no other industry is. Theories and philosophies about the origin of human life upon this earth, and opinions and beliefs about the divinity of humanity vary as widely as views can vary on anything, but there is one thing on which everyone must be agreed: that whatever may be the amount of divine authority by which men were created to be men, they were commissioned to be farmers by exactly the same amount of authority. We know that our Victorian predecessors had some devastating disputes arising from what to most of us now will seem very strange views of the origin of human life. It does indeed seem strangely tragic from our outlook to-day that differences about the origin and foundations of human life could have caused men to exile themselves and commit professional suicide and that any man of great ability should, as Sir Edmund Gosse wrote of his father, have burnt 'his ships, down to the last beam and log out of which a raft could have been made'; but even in differences so profound and acrimonious as those, there could not have been and there never can be any difference of opinion about the fact that the first thing men were commanded to do, or, if you prefer it, found that they had to do, was to take dominion over the things that live in the earth and the sea and the air in order that they might be cultivated and become the food of the people. It is in following this that men have established the interest and amenities of human life in a way that has differentiated human life from animal life. It is in following this far more than in any other experience of mortal life that human character and the qualities of courage, vision and patience have been developed. We cannot live on uncultivated nature and our primary duty as human beings in this physical world is to take uncultivated nature and produce from it something of a higher order which makes food for men as distinct from animals. It is not appreciated even by many educated townsfolk how fully and completely and altogether farming is a continuous and honourable fight against nature; that it is a continuous and strenuous opposition to the path of least resistance and therefore parabolic of all that is best in human character. Where nature would grow an Oak Forest the farmer grows Wheat; where nature grows a thistle the farmer is armed with a knife; whereas

nature intended a bird to lay a few eggs for the purpose of reproduction only, we have increased and improved the production so that we take the eggs and poach them and fry them and incorporate them in our cakes. The products of the farm are the spoils of war and we partake of them in the manner of those who celebrate a victory.

This fundamental fact seems to have little or no place in the general education of townsfolk. Nor do they fully realise that the victory is not continuously and always with men. Nature sometimes scores. The attack on nature has sometimes been taken with too little vision and forethought. The natural cover has been removed without appreciation of the peril to which the soil is exposed and from which it must be protected by some human device and perhaps there are few more serious problems in the agriculture of the world than the protection of farm soils from erosion.

Men think little, in taking their food, of the fundamental greatness of this warfare, of the way in which these good things have been fought for, and of the uselessness and sometimes the harmfulness of nature itself. They understand within limits only what is meant by the common saying that 'An apple a day keeps the doctor away,' but they do not fully appreciate the fact that it is only true of the cultivated product for which the husbandman has striven and that daily doses of nature's original apple would have exactly the opposite effect !

Some years ago the President at that time of the National Farmers' Union made a speech in which he complained very much of the agricultural character of nursery rhymes. He quoted them to show that they are nearly all agricultural, and accused the writers of them of deliberately trying to poke fun at farmers. That is certainly a complete misinterpretation of the facts. Nursery rhymes are agricultural because they cannot easily be anything else. The first literature and the first interests of mankind as a whole were agricultural and the first literature of every individual must be so too. There is no other industry that could contribute to a volume of nursery rhymes as farming does. No one could write a nursery rhyme that would be acceptable in the nursery about a modern industry. One has to turn to cattle and sheep and pigs and poultry and to corn and curds and whey for the industrial subject matter of nursery rhymes, and this is a matter of great significance. It means that the first education of everyone coming into the world, even in the largest of our cities, is agricultural education which unhappily becomes drastically upset in the majority of cases when the individual, in accordance with the requirements of certain Acts of Parliament, has to go to school !

But I must return to the consideration of war-time conditions. These did bring the vital necessity of the farming industry home to everybody and everybody was willing to add his own back garden to the resources and assets of the industry, but under these conditions of duress it was not the dignity and the high status of farming that was made manifest but its stark necessity, and necessity does not automatically imply prestige. In peace-time therefore people were so happy in their own immediate interests and in the use, as tokens of human welfare, of the products of the farm that they forgot about the origin of these things and failed to see the primary place of the farming industry in human affairs. In war-time, they were so concerned —not with the amenities of life—but with the grave danger of losing its very necessities and of facing starvation that they saw in farming nothing but its sheer necessity. Conditions of duress which bring home to the people the necessity of food production can never reveal the farming industry in its proper perspective, and I agree with Mr. Morison that the appreciation of this perspective can only be achieved in the course of time by the education of the people. Can a man, in the general sense of the word, he described as educated if he is without any appreciation of the status of farming ?

It would seem to be an urgent need in our scheme of general education to get people back to a knowledge of the fundamental and unalterable glories of human life. Later on this morning we have a discussion on the new shape of the Rural Community—a discussion which should be particularly valuable and to which I look forward, but at the moment I am concerned with the old and unalterable basis of the rural community which is itself the basis of the whole community, and I would plead for an understanding of farming and the farmer's way of life in the education of all people—an understanding which takes into full account that a farmer's duty is everybody's first duty, and that those who do not farm must recognise in all humility that they have delegated their responsibilities. I know that the suggestion that an appreciation of agriculture should be a matter of general education sometimes meets with the objection that it would tend to introduce something vocational into general education, but it is the whole point of my thesis here that in so far as farming is vocational there is a deep sense in which it is vocational to everybody. I do not see how anyone can object to an appropriate syllabus of agriculture in general education on the grounds that it predisposes people to become farmers any more

than they can object to the inclusion of English Language or English Literature on the grounds that it predisposes people to become writers, or, of religious knowledge, on the grounds that it prejudices people to church office. We are dealing in each case with something that belongs to everybody.

Before I leave this consideration, I must remove any possibility of misunderstanding on one point. In urging a revival of the recognition of the fundamental place and high status of farming, I make no implied criticism of modern industrial developments as such ; indeed, many of the modern industries have become indispensable in their service to farming, but the interests of modern industries concern a vast majority of the population who tend to forget that these industries are secondary. The economic repercussions of this have been dealt with in one of his books by Mr. Orwin, a former President of this Section, who points out that whereas farming should always be the industrial basis upon which other industries are built up like a pyramid, the pyramid has actually become top heavy and the structure stands more like a child's top than a pyramid. If in this connection I revert to my agricultural nursery rhymes I will do it without apology for they are as important, in their educational influence, to people in the nursery as our literature is to us. 'The House that Jack Built' has always seemed to me to call for special commendation because it is one of the earliest influences in determining orderliness of thought in a child's mind. It is the story of a great development, but the writer never allows a single stage in the development to be recorded without tracing it back through all the previous stages to the House that Jack Built which was the beginning of it all. The basis and the origin and the starting place are never forgotten. If from the basic industry of agriculture, industrial developments had taken place in that kind of way so that each stage in the development was properly related to its origin, I think we might be living in a different kind of world.

Farming and Science

I pass now to the relationship of science to agriculture.

While there had been experimental work on the growth of plants, some of which has become classical, in the seventeenth and eighteenth centuries and even earlier, the more general stimulus to the scientific study of agricultural material and processes was felt around 1840. The knowledge of the general way in which plants obtain carbon for their organic structure was established then for the first time, and after a little argument and empirical experiment the source of plant nitrogen became known. At this time the English Agricultural Society received a Royal Charter and addressed itself to its motto 'Practice with Science.' Some plot experiments had been laid down in other countries before this period but in this country they began with the Rothamsted experiments in 1843.

The rapidity of the work of scientists in agriculture produced a certain measure of cleavage which is still present to a considerable extent between those farm folk on the one hand who have more faith in what can be learned by experience than in anything chemists and other scientists can tell them, and those on the other hand who were prepared to seek and use the help of scientific experiments. Now this cleavage has, in my view, been a very salutory thing. I know there have been incidents in which people on each side have irritated people on the other and that some quite strong things have been said and written. These are, however, only incidents. In the development of the last century it has on the whole been a good thing that the importance of tradition and experience has been the emphasis of some, and the importance of scientific knowledge, understanding and thought has been the emphasis of others. It would certainly have been a serious matter if the whole farming community had stubbornly refused to accept the view that science could be of service to farmers, but I tremble to think what the consequences would have been if the farming community had placed itself entirely under the direction of the scientists ! The relation and co-operation of practice and science could not have been established in a day, and in the meanwhile it is a good thing that the fundamental importance of experience has been firmly maintained, particularly so as scientific study in the realm of farming—which is only a youngster compared with farming itself—has in the early period of its development occasionally suffered from its own exuberance. There was, particularly in the early part of the last century, a tendency for scientific people to offer plausible scientific explanations of agricultural phenomena without putting them to a proper scientific test. It has been a frequent subject of comment that after Thomas Way had published his classical paper which marks the beginnings of soil chemistry, Liebig made some devastating criticisms in which he repudiated the experimental findings of Way without himself submitting them to experimental test. If this sin is particularly noticeable in Liebig's career it is only because Liebig was a particularly noticeable man; he is by no means the only writer in the sphere

of agricultural chemistry who has in unreserved language laid down an explanation which a careful test would show to be quite untrue. It is difficult to quote many other instances because other writers are not removed from us by so many generations as is Liebig ! but soil chemists will know what I mean when I say that there is quite a lot of this to be found in what used to be written about soil acidity and the effects of lime upon the soil and about the availability of plant food. Happily, however, agricultural science has outgrown the over-exuberance of its youth and is more conscious now of the need for rigorous test and careful judgement.

The proper co-operation between the farmer of experience and the man of science has sometimes been effected with remarkable efficiency in specific instances involving the personal qualities of particular individuals. The late Sir Daniel Hall once recorded that in the days of his Principalship at Wye he was refused a hearing when he suggested that he should address the Canterbury Farmers' Club on the work of his College, but it was not a great many years after that that Hall was actually president of the Club. While the quality of the individual concerned must always be an important factor in the way things are done, there should be a very clear conception of the actual relationship of science to experience in farming and there should be enough experience of the relationship to go on now to make such a conception possible.

The contribution of science to agriculture has now become quite indispensable and the potential developments of agricultural science are enormous but if we look at the matter aright, I think we shall see that farming still remains fundamentally an art and a craft, and always will be. The earliest and most important developments of the work of chemists and others in the realm of agriculture involved the laying down of field experiments of which the Rothamsted experiments are classical in this country. I take a second place to nobody in my estimate of the great value of this work or of the increasing value of this type of work that has been made possible by the help of the statistician, but I would point out that in its essence it is only a continuation in a more orderly way of the acquiring of knowledge by experience which has been going on all through the ages. By arranging carefully thought out plots and making systematic observations on the crops we can ascertain that applications of certain substances to the soil have certain effects upon the plant. By doing this, however, we are only getting the same kind of knowledge that men had been accumulating more slowly in their experience. We are not getting a different kind of knowledge, we are only getting it more quickly. We are not in this way getting a more fundamental understanding of what is going on. These experiments are an expeditious extrapolation of the more haphazard way of acquiring knowledge. As a primary development they are not developments in agricultural science at all but developments in applied statistics. While a great deal of valuable chemical analytical work has accompanied field experiments, the field experiment as such involves practically no more knowledge of chemistry than farmers have had from the beginning. The Roman farmers acquired considerable knowledge about the use of lime in dealing with certain types of infertility, and those of us who are soil chemists might note—with some degree of humility—that no Roman farmer had ever been instructed in the meaning of *p*H or knew anything about the titration of acids by alkalies.

There is of course a wealth of agricultural science apart from empirical field experiments. It is significant, however, that this work, at any rate in relation to crop husbandry, has been least intensive in the most fundamental matters and most intensive in connection with more recent developments, and that is why we are still so dependent upon the empirical experiment. We have a considerable knowledge of genetics and plant breeding, there is much work done in the problems of plant pathology, bacteriology, mycology and entomology, the long-standing science of botany has brought detailed knowledge of the plant and the much more recent science of pedology is bringing new knowledge of the soil, of its formation, its morphology and the structure of its materials. But the basis of it all is the relationship of the soil and the plant and that is an almost unexplored field.

The neglect to study the soil-plant system is largely due to one of the tacit assumptions of the early agricultural scientists to which I have already alluded—an assumption in fact that there was nothing particular to study. It used to be stated—indeed in some writings still is—that the root hairs dip into the soil solution and the root absorbs substances dissolved there just as in the case of plants grown in culture solutions. Some of us were able to show some time ago that the problem is very much more complex than that. The plant and the soil are not two separate things that just touch one another : the intermingling of their colloids makes them one system—the soil colloids on the roots cannot be removed without rupturing the roots, they have come to be a part of the plant, and what goes on in this soil-plant system is the fundamental thing in all farming, and we know

very little about it. I do not want to convey a false impression and suggest that the subject has been completely ignored—there has, for example, been some important work on it in California in recent years. But attention to the subject is recent and, having regard to its major importance, the number of people studying it is small.

In most of our modern industries processes have been worked out from known principles. The chief processes may give rise to secondary problems requiring theoretical study, but in the main the theoretical knowledge has preceded the process. It is the other way round with farming. It has worked for untold centuries, but how it works is still a question awaiting a large part of its answer.

The underlying necessity of experience in farming as distinct from knowledge and understanding will be revealed to anyone who, without any experience, settles down to read a textbook of Agriculture. It is no criticism of these books to say that they are, on some important matters, very uninstructive and very dull to those who do not already know something about it. If I refer, as an example of this, to an account of the preparation of land for turnips from one of the most reputable and authoritative of our textbooks it must be understood that I do so without the remotest trace of criticism. I do not know how, in a book, it could be improved. We are told that following efficient autumn cleaning, farmyard manure should be applied and ploughed in unless it is the intention to apply this in the row in the spring. The actual depth of ploughing varies from district to district. In some cases the dung is ploughed in with a shallow furrow and the land is cross ploughed to the full depth later in the season. In other cases a full furrow is taken the first time and the land may not again be touched by the plough.

That is something of meaning and interest to people who have grown turnips or similar crops but it doesn't and it cannot tell anyone who has never grown turnips how to start. Similarly, in the description of the types of the various breeds of stock, the accounts mean something to people who have some experience of stock judging—even if only a little—but to the unitiated the descriptions appear to be little more than amplifications of the statement that the animal must not be too long or too short, too big or too small, and that it must be the right shape.

Farming still has a very personal quality. There are many ways in which the same thing can be done and a farmer's individuality and enterprise cannot be discounted in organisation and in the implementing of a policy.

The relationship of husbandmen of experience to men of science was brought home to me a few years ago by what some may think a very trivial incident. A group of gardeners came to the University of Leeds and asked if a course of lectures could be arranged for them. In making their request, however, they hastened to say—'We don't want you to try and teach us how to grow plants—we know more about that than you do—we want you to teach us how plants grow.' That exemplifies the relation of the husbandman to the scientist. The position is that *experience of husbandry is the first requirement, the second fact is that scientifically designed experiment can expedite the securing of that experience and in the third place science can bring considerable understanding to agricultural problems which adds both interest and usefulness.*

Scientific knowledge may be used in any industry by consultation with suitably trained scientists acting either as external advisers or whole-time employees, or by the scientific training of the managers in the business, or by a combination of the two. The two are certainly needed if science is to render anything like its maximum service to farming, and so I pass on to advisory work and then to the education of the general farming practitioner.

Advisory Work

Not only is farming unlike other industries in the fact that its successful practice long preceded the understanding of it, which is still only very partial, but it differs too in the fact that the scientific study of the industry involves almost every branch of science. Most technical industries are based upon one science, frequently chemistry, or perhaps two, with any others in a very subsidiary position. In the science of agriculture, however, chemistry, physics, geology, botany, bacteriology, pathology, zoology, entomology, veterinary hygiene, are all involved in a primary kind of way, and economics, while not a fundamental science, is also of major importance in a different sort of way. No farmer can either himself be qualified in all the sciences implicated in his work or employ people who are.

In recognition of this position and of the fact that farming is everyone's industry, the Provincial Advisory Service in this country was established soon after the first war, and every farmer had the right to call for the advice of specialists who were appointed to the staffs of various agricultural teaching institutions for the purpose of helping him with his problems. I assume that the details and developments of this service as it functioned between the two wars are well known, but there are two things about it which need to be stressed.

In the first place, the official view of this Advisory Service would regard it as a service rendered to farmers at the expense of the community. Actually there was an equally important, if not officially recognised, reciprocal service : the connection of teaching institutions with the problems of the day on the farms of their provinces, was an inestimable service to those responsible for teaching agriculture and the agricultural sciences. What has been said about the fundamental position of experience in agriculture and the inevitable limitations to the textbooks of agriculture will make it evident that the successful teaching of the subject is dependent upon a live contact between the teacher and what is going on on the farm. Most unhappily this service suffered a grave catastrophe as a result of war-time conditions. In the strenuous efforts to secure maximum food production from the land of our country technical advice to farmers was organised and implemented through the War Agricultural Executive Committees and it was undoubtedly organised very efficiently and in a way that brought credit to everyone concerned with it as a war-time effort, and the acceptance of technical advice to be acted upon was virtually compulsory upon farmers. The development, however, in this use of technical advice under abnormal conditions was abnormally rapid. Its achievements were obvious to everyone, and this was undoubtedly an important determining factor in the establishment of the National Agricultural Advisory Service, which divorced the service from the teaching institutions and badly upset the balance between experience, science and education. As a result of the abnormal conditions of war-time, the whole service became like a field of winter-proud wheat, and the ultimate consequence is difficult to contemplate. Those now engaged in this service have no official colleagueship with those who teach agricultural students in universities and colleges, and the teachers have no longer their foretime association with all that is going on in the farming of the day. These teachers are, therefore, deprived of what has been the main source of their life blood as teachers, and the suicidal character of the whole development can be seen by anyone who gives thought about the training of those to be appointed to the Advisory Service staff in the next generation or so. This separation of teaching and advisory work is in my view a major tragedy in the history of agricultural education.

There is a second observation to be made about the Advisory Service, either as it was or as it is. The service is fraught with a certain danger of being regarded as a substitute for the personal education of the practising farmers. On the very surface of things it may appear to the unthoughtful that since information is provided at much expense with a large establishment of specialists in the various branches of husbandry and in the various agricultural sciences, the farmer need not concern himself with any of these technicalities ; in fact, of course, the real ultimate success of an Agricultural Advisory Service must depend upon the intelligent and interested co-operation of the farmer in making use of the advice given to him and of being able to discuss his problems with members of the Advisory Service, a very different and deeper thing, be it noted, from a utilitarian knowledge of the subject matter. In this connection it might be noted that during the last century there has been an appreciable change in the relationship of farmers to the development of experimental agricultural studies. In the first days of the Royal Agricultural Society it does not appear to have been envisaged that all this experimental work should be carried out by scientists specifically appointed to do it as a whole-time occupation, but that the development of practice with science would be something that farmers themselves would take part in from the beginning. It is a significant fact that in the first ten volumes of the *Journal of the Royal Agricultural Society of England* practically every paper is written by a farmer or a landowner describing his own experiments and his own experiences on his own farm. In the last ten volumes of the journal every paper without exception is written by a scientist in a technical appointment. This change is in general a right and proper development always provided that all who work on the land maintain an educated interest in the whole of it, and this is the limiting factor in farming to-day. If it was true thirteen years ago, as I said at Blackpool, that the education of people on the land lagged far behind the scientific study of agriculture, it is still more so to-day following the abnormal war-time stimulus to agricultural science. So I come to the education of those who work on the land.

Agricultural Education

For many years I have had an increasing conviction that the agricultural educationists of this country owe an apology to the public in general and to farmers in particular for a certain lack of courage. This timidity shows itself in two ways :—

First, there is a kind of respectful fear of the fact I have already stressed that competence in practical farming is fundamentally the outcome of experience. It has been said again and again that anyone who is expected to

make a useful contribution to agricultural education must be able to talk to a farmer on his own level : that there will be no confidence in anything chemists or bacteriologists are saying and doing unless they can teach the farmer everything about his farm. I have always agreed that *scientific advice* to a farmer on a specific farm problem should go through a husbandry adviser and not be given direct by the specialist since it must be related to the whole conspectus of farm work and policy, which the specialist as such cannot be expected to do. A specialist may quite correctly say that a certain treatment will mitigate a disease or some other crop defect, but he cannot say whether it would not be better to re-arrange the cropping to avoid that crop altogether. It was the intention that the former provincial advisory service between the two wars should work this way, the general agriculturist and not the specialist being the final adviser, but on account of certain properties of human nature it seldom did work that way. Section M was discussing this very matter and seemed near to the point of making some practical suggestions in the matter in Dundee in 1939 when the bombing of Warsaw turned our thoughts to other things. But in matters of research and education, the idea that a specialist dare not go on to a farm to investigate a problem or that no one should pretend that he can make any useful contribution to agricultural research unless he can stand cross examination on everything that is going on on the farm is ridiculous. Nor should any scientist or educationist be intimidated by this idea : farmers are not ogres, they are a kind-hearted and understanding class of people.

A second evidence of our fearfulness is a disabling lack of frankness and forthrightness in admitting that agricultural education is education and nothing but education. There has been a ' jam-and-pill ' policy which is really unworthy of any educationist and which is far from complimentary to the members of the farming community. Of course a subject must be presented in an interesting and attractive way, but the idea that there can be no interesting and attractive way which is not based on an obvious financial advantage is an insult.

Having stated that agricultural education must be education in all its reality, I am compelled to remark on a few features of education as I see them and as I try to use them for purposes of agricultural education. To the professional educationist they may well appear to be inadequate or elementary and I do not presume to speak of education in any kind of perspective. That indeed I could not do, for the administrative arrangements and the legal enactments concerning general and technical education to-day have become so very elaborate that it is very difficult for anyone to whom it is not a whole-time occupation to know and understand it in all its detail. Personally, in sheer fear of the elaboration and for my own purposes in agricultural education, I have gone right back to the House that Jack Built and to those fundamental things about education which no committees and no education officers and no Acts of Parliament can ever alter. Here one finds that education is a very personal matter concerning the discovery and development of the qualities of mind and personality of people, no two of whom are exactly alike. Long before any Act of Parliament required compulsory attendance at school (I think the frequent phrase ' compulsory education ' is wrong somehow) people did become educated but often without any organised schemes to assist their education. The coming of organisation involving the appointment of teachers and the enrolment of pupils is really an expeditious continuation of the older and slower processes, in very much the same way that the designing of plot experiments is an expeditious continuation of learning about farming by experience.

Going back to these simplicities and looking at the position to-day, I am inclined to wonder whether there may be a tendency for education in all its reality to be partly suffocated by its own administration and to be misled by its terminology. The vital things that matter to me in education involve the personal qualities of the teacher, the personal qualities of the pupil and the personal relationships between them, and all matters of administration should be subservient to that consideration and designed to increase the possibility of the development of these personal matters. If too many people have something to do with arrangements and administration concerning education who are not engaged in the process itself and who sometimes have no experience of it at all, there may be some difficulties.

That there is from my point of view a tendency for our conception of education to be led astray by terminology seems to arise on two points. The first concerns the use of the words ' Pass ' and ' Fail ' in connection with examination results. While much has been said about our examination system, I would point out that one important consideration which does not often seem to arise for discussion is the interpretation of the results of examinations. To say that a man who has not reached a certain attainment in a certain subject has failed in that subject seems to me to be quite a misleading way of looking at it. He cannot possibly have failed in any real sense of the word unless he failed

to make the serious attempt. If he has made the serious attempt and finds that he has no aptitude for the subject, then he has succeeded in discovering that much about himself, and to discover what he cannot do is as important as to discover what he can do. If after serious effort a pupil finds that he is quite incompetent in Latin and Greek, the way to put it is not that he has failed in these things but that he has discovered that whatever his duty in life is he will not be called upon to play a prominent part in preventing the dead languages from being interred. The early stages of education must surely be considered primarily from the point of view of the discovery of the latent and inherent abilities of the individual and the later stages to the development of those particular aptitudes which have been found to be his. I am sure there is an idea in the minds of many men in the street that there are certain subjects which form the basis of general education and that a person is educated or uneducated according to his competence or incompetence in these subjects. I would rather regard these subjects in the early stages of education as the means of discovering the qualities and abilities of the individual. Where that has been done, the subjects that were used may cease to matter. The tools may be forgotten when the task is finished.

But there is a second point of terminology which I think has confused some people's thinking. The frequent use in the past of the terms ' advanced education ' and ' elementary education ' has always seemed to me wrong. I know this terminology is much modified nowadays, but there is a well-established idea that differentiates between elementary and advanced education whatever words may be used. Surely it is the subject matter that is either advanced or elementary and not the education—not the discovery and development of personal qualities. The differential calculus is relatively advanced and the multiplication tables are relatively elementary, but does anyone seriously suggest that the education of a youth of 16 or 17 is something more onerous or difficult or in any way more ' advanced ' than the education of a boy or girl of 6 or 7—if there is any difference I should think it is the other way on. The same consideration applies to different types of people of the same age engaged on different kinds of study because they are different kinds of people.

There is one further general remark about education before I come specifically to agricultural education. I always consider the primary purpose of techical education as the training of people to be interested in the subject of their choice. There are men of business who on the face of things are inclined to resent the suggestion that a scheme of technological education should have as its avowed purpose the development of interest and the study of interesting things because they are interesting. Even, however, from the purely business point of view I fail to see how there can be any more reliable basis. A man who is really interested in the work with which he is concerned can assuredly be relied upon to work more efficiently. The attempt to develop efficiency as the fundamental thing will always meet with some measure of failure, but the capacity to be interested—always assuming it is associated with the necessary discipline of life—will automatically bring efficiency. The primary purpose in technical education is not to train people to put their brains into their business but to put their hearts into it and if their hearts are there their intelligence will follow.

The general programme of education as I choose to look at it begins with the emphasis on the discovery of the individual. With increasing knowledge of this, the emphasis passes to the use of appropriate subjects for the training of powers of thought and understanding, of mental discipline, and of the capacity to be interested ; and then, in vocational training, there is the further development of these qualities with a particular development of interest in matters concerning the chosen career.

Applying these simple ideas to agricultural education, I should like first of all to get rid of the conception of ' elementary ' and ' advanced ' education and of ' lower ' and ' higher ' levels. No one with any experience in agricultural education can be unaware of the idea in the minds of many people that there is a certain ideal ' level ' or ' standard ' —presumably that of the Universities—and that there is something to be regretted about the fact that many are not able to study at this ' level.' Perhaps this has never been said explicitly—if it had been its absurdity might be more obvious—but the existence of the idea is beyond all question. It is implicit in the frequent statement in letters from schoolmasters and parents—' unfortunately this lad is not likely to reach University standard '—it is there quite definitely if the word ' unfortunately ' has any meaning.

This idea has in the past led some institutions to try to do work different from that for which they were intended, designed and equipped. Colleges admirably equipped to do College work and to conduct diploma courses have tried to give University degree courses for which they never were intended, designed or equipped. The responsibility for this must be divided, in proportions I am quite unable to assess, between the College people with a false sense of prestige and a

University that gives an external degree. I recall too that I was once reproached by the principal of one of our institutions because he said I had referred to his institution by what he called the 'opprobious epithet—Farm Institute.' Oddly enough there has been an opposite tendency whereby University Departments of Agriculture, fearful of being thought too academic and unpractical by the farming community, have tried to work more like technical colleges and less like universities, and to this I will refer a little later in another connection. All this is wrong. Prestige is always in danger when those concerned are making conscious effort to get it. If an institution will do the work it is intended to do in a faithful way its prestige will look after itself. A farm institute can be a farm institute, and a technical college can be a technical college with just as much dignity and prestige as a university can be a university, but if any one tries to be another for which it was not intended then its prestige will be in danger. It is of course possible, and has in the past been done with advantage, to integrate more than one type of institution in the same administration, but that is quite another matter, and in any case is unlikely to happen very much in the future.

Farming is far more heterogeneous than any other industry. It has a great diversity of crafts and implicates a multiplicity of sciences. People are therefore interested in very many different ways—at one extreme in purely practical ways, at the other in very scientific ways—and with every possible intermediate combination of practical and academic interests. Each is entitled to have his interest developed according to his own line of approach and each is necessary to the industry. One kind of educational provision is needed for the man who will have to decide what is to be done and another for the man who has to do it. But they should not be courses that are differentiated in any *significant* way as 'advanced' and 'elementary.' They are different kinds of courses for different kinds of people and that is all there is about the difference between them.

In determining the kinds of courses that should be available, the present chief types of institution, Farm Institute, College and University, provide, so far as they go, a reasonable basis for this, but three matters seem to call for attention.

It is very necessary that the three different types of established agricultural education institutions shall be clearly defined as what they are. The farm institutes are I think quite clearly defined, but as I have already indicated there is a sort of blurred overlap between colleges and universities, the colleges having a tendency to be more like universities than they should be, and the universities tending to be less academic than they should be for fear of criticism from practical people. In deciding what is the appropriate institution for certain individuals to attend, there are certain border-line cases in which it is not easy to determine whether the individual should attend a college or a university, and undoubtedly some people have entered upon university degree courses who would have been more appropriate in the diploma courses of the colleges and vice versa. If, however, the character of the college and of the university are not clearly defined these border-line cases will be very much more difficult to deal with because no one will know where the border line is. The University Departments of Agriculture are mainly responsible for such difficulty as exists in this matter, and I will refer to their position in a little more detail at the end.

In the second place, there is room for investigation and experiment in devising some new types of educational programmes for the more practical minded people. That is no criticism of farm institutes, and I have no criticism to make of them. The point is that even with Farm Institute developments in every county they will be insufficient to provide for the agricultural education of all the people working on the land, and many of these in any case would not be appropriate as pupils in a farm institute or wish to attend a residential course. Of the 750,000 people working on the land in England and Wales, it may be assumed that in any one year something of the order of 15,000 are at an age at which some appropriate course might develop their technical interest in agriculture and clearly 50 farm institutes in the country cannot cope with that number, even if they were all suitable to be farm institute pupils —which is not the case. There are voluntary organisations, notably the Federation of Young Farmers' Clubs, that have rendered invaluable service in the development of informal education for people entering the agricultural industry, but there is scope for a good deal more provision of some kind of day or evening classes in which there is an appropriate balance of discipline and informality which will bring certain types of practical farm workers, who would be quite inappropriate to courses of lectures in agricultural science, into useful contact with scientific developments. This admittedly sounds a little vague and I might give one practical illustration of the type of person for whom I think some new scheme might be devised. Several times in the past I have been concerned with people, who were clearly born farmers, taking a diploma course in agriculture and agricultural science who have shown

themselves quite incompetent to deal with the first examination hurdle. They have faced the fact of their complete incompetence to write an intelligent answer to any questions and have presumed that the only thing to do is to withdraw from the course. In the absence of any alternative course to which they might go, I have sometimes advised these people to continue with the course and not to worry about the examinations. I am profoundly convinced that continued attendance was of value to them and did a good deal to develop their interest in the developments of agriculture and agricultural science and that it made a contribution to their subsequent farming careers, but I am equally convinced that it was very much of a second best and that there should be some other type of course for people of that kind.

There may be some technical subjects for which a limited number of courses is provided and for which those unable to cope with any of the courses are regarded as unsuitable. That is not so with farming. There are born farmers who could make no orthodox success of any of the existing courses. In other technologies compliance with the requirements of a course may be a necessary part of proving a man's fitness for the subject—the man must comply with the requirements of the course or go out. In farming, however, there is far more need for courses to suit the people rather than people to suit the courses. The study of individuals, their potentialities and needs, and the study of new types of courses ought to be a very fruitful field.

Another aspect of this matter merits attention. It is often regretted that so few of those who take one of the existing courses become practical farmers. Surely, however, the point to note is that the percentage of courses suitable for people going into salaried posts is much higher than the percentage of agriculturists who take up salaried positions. More and different kinds of educational facilities are needed for the bulk of the practical workers to establish their harmonious relationship with farming, science and education.

Finally, I would like to refer again to University courses in agriculture. We all know that the introduction of agriculture into some of our universities was in different ways resisted. It was at some centres resisted from within the University because of the fear that so practical a subject would be inappropriate and would vitiate the academic atmosphere of the University. It was also resisted by certain members of the farming community on the grounds that it would tend to make people concerned with agriculture too academic and unrealistic. Since the days in which these objections were raised agriculture has become firmly established in many of our universities, but I have never heard the question raised in the light of the experience we now have, as to whether these early apprehensions were to any degree justified. In my own opinion the facts have proved that there was some measure of justification for both of these objections. The plain fact is that agriculture *can* be dealt with within the universities in the appropriate university way, but that for fears to which I have already alluded it is not altogether thus read. One crucial test of the proper way of reading a subject in a university is that truth is being studied for its own sake and any utilitarianism that may arise shall be exclusively a by-product. There is no reason at all why agricultural material and processes should not be studied in this academic way and it would be wrong to suggest that it is not so studied to a considerable extent. We may take note of the fact that the study of agriculture in our universities is largely responsible for the development of pedology as a non-technical science. That is one point of evidence that to a certain extent agricultural subjects have been studied in the way appropriate to a university—but only to an extent. The insidious incursions of the less fundamental and utilitarian motives are doing considerable damage. There is always the tendency to introduce into the agricultural curriculum everything that is likely to be ' useful ' to a general agriculturist. Complaints about the consequently crowded time-tables, the lack of time and opportunity for reading and reflection, and for association with other subjects and activities in the university have been forthcoming for years but no one seems to have the courage to do anything about it. The conception of agricultural studies as a discipline seems to be a theoretical ideal which has been abandoned in practice. It seems difficult to persuade those engaged in agricultural education in our universities that the importance of the usefulness of the subject matter of the moment is not a thing that matters in itself. We must use current subject matter in our educational work but we should remember that it does not remain current subject matter and that it is used to train personal and individual qualities of thought and judgment which can be applied to new and unforeseen subject matter that arises in later years. For many years past now I have found that in setting degree examination papers I never set a question which I could have made the least attempt to answer at the time I graduated—I knew nothing about it then and for the most part nobody else knew about it either—and many of those things of which I did then

know something I have had little to do with for a very long time. It is not the subject matter of our undergraduate days that matters in itself, and I am profoundly convinced that the universities will infuse a more valuable influence and better leaders of thought into agricultural affairs when the agricultural curriculum has been drastically reduced, with more time for students to be influenced by the indulgence and discipline of university life as a whole.

There are some educationists who are demanding, on account of the needs of the day, that universities shall function more like technical colleges and shall co-operate more with them. This is impossible. Anyone may say that a university is not an appropriate place for agriculture, but what he cannot do is to say that the university shall treat the subject in a non-university way, and still call itself a university.

The universities can treat agriculture as a university subject, and produce men with considerable understanding of it, and the universities will have to resist the temptation to utilitarianism and produce such men who in the long run will render far greater service to the industry and to the community than those merely trained as agriculturists. The very odd thing is that agriculture would get far more from the universities if it expected less. I know there are some who, in the interests of university idealism, would like to see such a subject as agriculture removed. I submit on the contrary that agriculture is a subject, which, if it has the courage and statesmanship, can take the princely action within the universities that will see them through the alleged crisis of these days and reaffirm their true definition.

In the development of technical education no technology called for so much attention as agriculture and none received less. Travelling difficulties and the far-flung locations of the farming people was one, but only one, of the difficulties. The way is now fairly clear for education to render its full service to farming, and I have in all humility suggested these thoughts about farming, science and education because I cannot see how any agricultural policy can be pursued unless the importance of each, and the relative importance of each, is fully heeded, and unless the education and interest of the people is given its first and rightful place.

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